

OBITUARY NOTICES

EMERITUS-PROFESSOR JAMES DREVER, M.A., B.Sc., D.Phil., F.R.S.E.

By the death of Emeritus-Professor Drever on 10 August 1950, psychology sustained an immeasurable loss. All who were privileged to know Prof. Drever realized the influence he wielded in moulding the psychology of his day and generation, and were aware of his very considerable international reputation. Prof. Drever was very conscious of the honour conferred upon him by his election as President of the Twelfth International Congress of Psychology. It was a disappointment that in 1948 he was unable, for health reasons, to officiate at the meetings in Edinburgh, which had been postponed from 1940 because of the outbreak of hostilities.

The late Prof. Drever was born on 10 April 1873, on the Island of Shapinsay, one of the Orkney Islands off the north coast of Scotland. His early years were spent in Orkney, and he received his schooling at Stromness Academy. He was proud of being an Orcadian, and no one was long in his company without some reference to the island of his birth. His early days imbued him with a love of the sea, an interest which persisted throughout life, and led him to become an expert yachtsman.

Prof. Drever began his career in the teaching profession, and was a member of staff of different schools, acting first as assistant, and later as headmaster over a period of ten years. He was then invited to join the staff of the Education Department of Edinburgh University by the late Prof. Darroch, then Professor of Education. After this appointment, his first written work began to appear. This took the form of an occasional article in a scientific journal, and a small book on *Greek Education* which covered the topics of one of his lecture courses. The establishment of a small pedagogical laboratory in 1912, one of the first in the country, marked the beginning of a new era for him. This laboratory was set up in Moray House Training College for Teachers, Edinburgh, and Mr Drever, as he was then, was made its first director. The apparatus was meagre as judged by present-day standards, but included, *inter alia*, a Hipp chronoscope, an ergograph, memory apparatus, etc., and was intended for the use of Honours students in Education. This led to the inauguration of research on children's vocabularies, and to analytic studies of school subjects such as reading and writing. A writing pen was devised by means of which pressure in hand-writing could be measured. Towards the end of this period *Instinct in Man* was written. This original dissertation forms one of Prof. Drever's most important contributions to psychology. It reveals his philosophical approach to psychological problems, and contains for the most part a critical and analytical discussion of McDougall's instinct theory. This was submitted for the degree of Doctor of Philosophy (D.Phil.) which was awarded him by Edinburgh University in 1916.

It was at this time that the transition occurred from the Department of Education to the Department of Psychology. Prof. Drever was appointed lecturer in psychology at Edinburgh University, an appointment which carried with it headship of the department. This was a small department of the University, but it housed, nevertheless, a first-rate laboratory which had been founded by his predecessor, the late Dr W. G. Smith. Under Prof. Drever, the department gradually developed until it became one of the largest in the country. The recognition of his abilities by his University was shown by the inauguration of a Chair of Psychology, the first to be established in Scotland, to which he was appointed in 1931, and which he continued to occupy until his retirement in 1945.

The esteem in which the George Combe Psychology Department of Edinburgh University is held is largely due to the enthusiasm of the late Prof. Drever. All members of his staff were aware of his high standards, and of his belief in, and devotion to, the science which he had made his own. He was a hard worker, and untiring in his efforts. He disliked holidays, or any break which interfered with the routine of work. Nothing gave him greater pleasure than to puzzle out intricate electrical circuits, and many Honours students will recall the setting-up of apparatus involving elaborate connexions unintelligible in the main to them, but affording delight to the head of the department. Occasionally, he became the characteristic 'absent-minded Professor' as when he arrived at an evening function a week after it had taken place, or a week before it was due to occur, or when he hurried off to a committee meeting only to return shortly afterwards with the information that he had mistaken the day!

One of the outstanding traits of his character was to do well anything he attempted. If he agreed to write an article for a journal, or to deliver an address to an outside body or to serve on a committee, one knew that it would be done efficiently, whole-heartedly, and with earnestness of purpose. He had a high standard of achievement which inspired both staff and students. He became impatient with slipshod work, and only the best would satisfy him. It is small wonder, therefore, that the Department of Psychology of which he had charge became gradually one of the most important in the country. The number of students increased, and this in turn led to the provision of extra lecture courses, and to the appointment of additional members of staff. Students and research workers from other countries were attracted, a tribute to the inspiring lectures delivered, and to the recognition of the intrinsic value of the courses provided. During his tenure of office, three degrees in psychology were instituted; Bachelor of Education, a post-graduate honours degree in psychology and education; Bachelor of Science with honours in psychology; and Master of Arts with honours in psychology.

The Department of Psychology developed primarily as an experimental department. Prof. Drever was convinced of the value of approaching the study of psychology from the experimental angle. All students, whatever their year of study of psychology, were compelled to spend a certain amount of time in the laboratory. Even first-year students of psychology carried out elementary experiments, and all honours students were required to spend six hours weekly on practical work. This emphasis on practical work gave the student an insight into the technique of experimentation, and many former students now engaged in many different spheres of psychology and education, have expressed in later life their appreciation of this training.

Prof. Drever was a pioneer in many ways. He realized at an early date that psychology was more than theory, and that many of its findings and methods of investigation could be applied in practical fields. One of his earliest books was written on *The Psychology of Industry*, a book which had considerable publicity when it first appeared. His interest in the vocational aspect of psychology led to the initiation in Edinburgh of a scheme for testing apprentices in the printing industry. A battery of psychological tests was devised, and every potential printer was required to appear at the psychology department for examination. The results were graded and then reported to a committee of employers and employees who considered them in conjunction with their own assessments. This scheme has now been in existence for twenty years, and has spread to other parts of Scotland, and to different centres in England.

As far back as 1925, Prof. Drever became interested in the problems of delinquency

and inaugurated a Child Guidance Clinic in Edinburgh. The staff was voluntary, composed mainly of members of staff of the Psychology Department. The clinic met a felt need in the community, and soon other specialists were offering their services, psychiatrists, pediatricians, social workers, play-room attendants, speech therapists, etc. The clinic, which began in a small way, became soon an important centre with separate premises. Advantage was taken of the facilities offered, by the Education Committee, by the school medical officer, by social workers, probation officers, headmasters and head-mistresses, etc., and all whose work was associated with children. The clinic continued in existence for many years until the work became too heavy for its voluntary staff, and it was superseded by an official Child Guidance Clinic sponsored and financed by the town.

Other activities undertaken by Prof. Drever were studies of problems associated with handicapped children, particularly the deaf and the blind. At the request of a headmaster of an institution for the deaf in Edinburgh, Prof. Drever initiated an investigation into some way of assessing the mental ability of the deaf child. This investigation was supported by the teachers of the deaf with great enthusiasm, and the research soon spread to other parts of Scotland in which deaf children were housed, and to all similar schools and institutions in England. This led (with the collaboration of his colleagues) to the development and standardization of a battery of performance tests suitable for the testing of the intelligence of the deaf child. Later, norms were established for the same battery for the hearing child.

Prof. Drever also studied many of the problems of the child suffering from the disability of blindness, and for a number of years he acted as chairman of the Royal Blind Asylum and School in Edinburgh. His advice was invaluable in helping to guide the policy of the education and training of such handicapped children. This interest was transferred later to the inauguration of a scheme for a Scottish training centre for the war-blinded. Prof. Drever spent many hours and much energy in assisting the setting-up of this centre. He soon established himself in the affections of the blinded men, and they recognized in him one who would help them to overcome their difficulties in their new adjustment to life.

His other activities were manifold, and he served on many committees. He was a member of the National Advisory Council for Scotland on Physical Training, of the International Examinations Enquiry, of the Advisory Committee of the Directorate for the Selection of Personnel, War Office, of the Scottish Council for Research in Education, of the Scottish Association for Mental Health, and also acted as President of the Scottish Child Guidance Council. He was president of the Psychology Section of the British Association for the Advancement of Science in 1926, and president of the British Psychological Society during the years 1935-8. He was the first president of the Scottish Branch of the Society, the formation of which was largely due to his efforts. He was a Fellow of the Royal Society of Edinburgh and, in 1938, the King of Norway honoured him by making him a Knight of the First Class of the Royal Norwegian Order of St Olav.

Prof. Drever was also for many years editor of the *British Journal of Psychology Monographs*, and acted as associate editor of the *British Journal of Psychology*. He was also an associate editor of two American journals of psychology. His last publication, completed just before his death, and at present in the press, is a *Dictionary of Psychology*, to be published in two volumes in the Penguin Series, which will be a useful adjunct to the libraries of all psychologists.

His contribution to psychology is not easy to estimate. Through his writings he spread

a knowledge of psychology and of its achievements. One of his best sellers was *The Psychology of Everyday Life* which ran into many editions. Other books became standard text-books, and are well-known in other countries. His influence can also be traced more indirectly, in the instruction of many thousands of students who passed through his hands. He imparted to them his own enthusiasm for psychology, and his belief in its ability to help mankind. His former students are now, many of them, in important positions, directing policy in matters psychological and educational, both in this country and abroad. All are conscious of the debt they owe to him, and are appreciative of the thorough grounding in psychological principles which they received.

Prof. Drever will be remembered most for his kindly and unassuming manner, his willingness to help others, his many unobtrusive acts of kindness, his friendliness and his wise counsels. He had a quick and alert mind, and readily grasped any problem presented to him. He was tolerant in his attitude, and unbiased in his outlook. He was an omnivorous reader which gave him an all-round knowledge of many subjects. He was interested in languages and their construction, and was often to be found puzzling over the grammar of still another foreign language, in order to be able to read a scientific article in its original.

His colleagues and students in this country and abroad mourn the passing of a friend as well as of an eminent psychologist. Prof. Drever was one of the outstanding figures of his own generation, a scientist of the first order, and a distinguished scholar.

MARY COLLINS

H. M. VERNON, M.A., M.D.

Horace Middleton Vernon was born in 1870 and educated at Dulwich College School and Merton College, Oxford. At Oxford he graduated with 1st class honours in both chemistry and physiology, and as his research work he investigated the effects of environment and cross-fertilization on the growth of small marine animals. He qualified as a doctor but after his election as a Fellow of Magdalen College, Oxford, he became primarily a teacher of, and research worker in, physiology.

During the 1914-18 War he spent a vacation as a munition worker in Birmingham where he realized some of the difficulties and complexities of industrial problems. When the Health of Munition Workers Committee was appointed, Dr Vernon was asked to become an investigator, and when that Committee was re-organized as the Industrial Fatigue (later Health) Research Board, he continued his researches for that Board until he retired in 1932. He published a number of reports to the Board on absenteeism in iron and steel works, atmospheric conditions in collieries, heating and ventilation in factories, and accidents. In addition he embodied these and other researches in a number of excellent books including *Industrial Fatigue and Efficiency*, *Accidents and their Prevention*, and *Health in Relation to Occupation*.

He died on 11 February 1951, after a life spent in work which afforded him great satisfaction and continuous interest as well as success.

His work, concerned as it was with the human side of industry, necessarily involved him in psychological as well as physiological problems, and it illustrates how much can be contributed to their solution by those who are not psychologists in the academic sense of the word.

EXPLORING THE INTELLECT¹

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I. DEFINITION AND ASSUMPTIONS

For the purposes of this discussion I propose to define intellect as a cluster of high-grade skills concerned with problem solving. I make no assumptions about its structure, nor do I imply that it is a unitary faculty. I do assume, however, that a fruitful method of studying intellectual functions or skills is the measurement and analysis of individual differences in performance, and that testing, far from being (as Zangwill⁽³⁰⁾ has recently asserted) only a technology, is an important tool of research, which, via Mill's method of concomitant variation⁽¹⁷⁾ and its statistical derivatives, can reveal valid information about the structure of the intellect. I also assume that the intellect is worth studying. The reaction against cognitive psychology goes too far if it denies that the intellect is one of the most characteristic and important attributes of man.

II. OUR MAP OF THE INTELLECT

The question I wish to discuss is this: Is our map of the intellect more or less complete or is the intellectual continent still largely unexplored? On this matter the experts appear to disagree. Burt⁽³⁾, for example, asserts that 'probably all the more important broad group factors have been approximately identified'; while Vernon⁽²⁷⁾ believes that 'there is little ground for the supposition that there exist a number of intellectual faculties, of educational importance, which are not covered by objective intelligence and attainment tests'. On the other hand, Guilford, in his stimulating presidential address to the American Psychological Association⁽⁵⁾, states: 'It is probably safe to say that the typical intelligence test measures to a significant degree not more than half a dozen intellectual factors. There are surely more intellectual factors than that.' While to some extent this divergence of viewpoint is due to the differences between British and American methods of factor analysis, ably discussed by Vernon in the appendix to his book, it is probably to some extent due to the influence of national character. The British view is closed and orderly; the American view less tidy and still adventurous. One would indeed have been more sure of the objectivity of factor studies if British investigators had not discovered an hierarchical society of factors crowned by '*g*', and the Americans a democratic society in which all factors are created equal. In spite of the mathematics it is not certain that psychologists have abandoned the political model that Plato was the first to use.

I do not intend to discuss the technical merits of the British and American factorial schemes, but to present certain arguments of a general kind against the closed British

¹ The substance of this paper was delivered at the Annual General Meeting of the British Psychological Society in Liverpool on 29 March, 1951.

view that our map of the intellect is in its broad outlines more or less complete. The arguments for the finality of any scientific doctrine need to be of a most cogent kind before carrying conviction, because on other grounds there are strong objections to any such belief. The belief in finality is indeed heuristically inhibitory and contrary to the exploratory genius of scientific research. The scientist is unlikely to discover anything unless he believes that there is something to discover. The history of science, moreover, warns us that even in the more mature physical sciences proclamations of finality, even when apparently most firmly founded, have been falsified by the progress of research. It would therefore be rash to claim anything like finality in the psychologist's map of the intellect unless the arguments are logically overwhelming.

In actual fact the arguments are exceedingly weak. The contention that there are probably no intellectual factors but those already identified is no more logically sound than the contention of an eighteenth-century naturalist that there were no swans except white ones. Such a statement would only have been justified if the universe in which swans could exist had been completely explored. In the same way the psychologist is not entitled to assert that there are no major factors but the existing ones, unless he can show that the universe of intellectual abilities has been completely explored; that there can be no Australias of the mind. Is he prepared to make such an assertion?

Perhaps an even more important consideration is this: the flour the psychologist extracts from the factorial mill must depend on the grain he puts in. No verbal tests: no verbal factor. And this must apply to all actual or possible factors. So once again the psychologist has no right to claim factorial finality unless he is satisfied on other grounds that the whole range of intellectual functioning has been embraced by his tests. What can ensure that the universe of intellect has been fully sampled?—that test content is co-extensive with intellect itself?

III. TEST CONTENT AND THE ROLE OF THEORY

There is only one thing that can ensure that the universe of intellect has been fully sampled, namely, a systematic and coherent theory of intellect that has stood up to every empirical check. Against the background of such a theory we might have good reason for believing that our map of the intellect was in outline complete, and that our tests adequately sampled the whole domain. Do we possess such a theory? Quite clearly we do not. In fact about twenty-five years ago an almost total switch occurred from the qualitative study of intelligence to the elaboration of statistical methods of analysis. As Meredith⁽¹⁶⁾ has pointed out, Spearman's work on the nature of intelligence⁽²²⁾ has been largely ignored, while his factorial methods have flourished exceedingly. It is more than a quarter of a century since Thurstone wrote *The Nature of Intelligence*⁽²⁵⁾ or Thomson *Instinct, Intelligence and Character*⁽²³⁾. We have no adequate current theory of intelligence, and test content is determined partly by mere administrative convenience, partly by an analogy with practical tasks, partly by unco-ordinated intuitions, and partly by the vestiges of past theories. There is a sting in Zangwill's contention that testing is merely a 'technology'.

This almost total switch from theoretical construction to the elaboration of statistical methodology is for several reasons unfortunate. First, as Burt points out⁽³⁾: 'The sole claim of mathematical analysis should be to verify hypotheses commonly advanced on the basis of much broader general lines of evidence'. Secondly, the role of theory as an

ingredient in research is now more adequately appreciated, and has been stressed recently in this country, for instance by Thouless⁽²⁴⁾. It is paradoxical that theory should be eliminated from the study of intellect precisely when its general importance is being recognized. Thirdly, although there is no current theory of intellect designed to serve as a basis of testing, there is an accumulating mass of material that obviously contributes towards such a theory, for instance the neurophysiological material summarized by Hebb⁽⁷⁾ and the genetic studies of Piaget⁽¹⁸⁾. Until this material has been organized in some coherent form we can have no assurance that our tests are even adequate, much less that they are complete.

IV. SOME PATHS FOR EXPLORATION

It seems probable that the theory of intellect will be founded on, and derived from, learning theory. Learning is problem solving, though not necessarily, or indeed usually, on the symbolic level. Intellectual skills are learned skills (however much they depend on a basis of innate capacity and maturation), usually, though not necessarily, on the symbolic level; they are differentiated from learning in general by a minimization of repetition and a maximization of relevant generalization. From this very brief theoretical statement I propose to derive three paths for exploration, to show that these paths have not in fact been explored, and hence to demonstrate that the intellectual map cannot be regarded as complete.

(i) *The time dimension*

Perhaps the most basic intellectual skill is that of transcending time, involving the temporal integration of the stream of experience. I am not referring by this to merely reproductive memory, but to a sort of gathering up as one goes along, in the way, for instance, that one follows the thread of a story. Köhler⁽¹⁵⁾ noted that apart from lack of speech the short time span of the ape's experience was 'the chief difference between apes and even the most primitive human being'. It is agreed that the delayed reaction situation⁽¹²⁾ and the temporal maze⁽¹³⁾, both devised by Hunter, constitute difficult tasks for animals, since they involve temporal integration and possibly the rudiments of symbolic functioning. It would seem that intellectual skills are in their very origin closely bound up with the capacity to transcend time.

Now in all existing tests of intelligence the data of any given test item are presented simultaneously on a printed page, or, in the case of oral tests, within a very short time span. The need for temporal integration is eliminated or reduced to a minimum. Thus none of the fifty-six tests used by Thurstone in his search for primary mental abilities⁽²⁶⁾ involved a time dimension except, possibly, the rote memory tests: and in these it was a matter not of continuous temporal integration but of a two-stage process of presentation and recall.

This exclusion of the time dimension is rather surprising. In daily life nearly all problems are in a time stream and their solution involves temporal integration. In actual fact the solution of problems, the data for which are simultaneously present, is a rare case. The only place it is at all common is in the school text-book. It looks as though test constructors have been unduly influenced by the scholastic model. Why is it, moreover, that animals should have been presented with temporal mazes, but human beings never with temporal intelligence tests? Finally, philosophers from Kant onwards have emphasized

the close relation of mind and time, and Alexander (1) even asserted that 'mind is a form of time'. But as Wolters (29) pointed out, at the Twelfth International Congress of Psychology, psychologists have in general been guilty of a neglect of time.¹

I am suggesting, therefore, that as a rule intelligence tests should involve the successive presentation of data, and should bring in what would appear to be the basic intellectual skill of temporal integration; that temporal matrices should supplement or perhaps even largely supplant spatial matrices. At least this is a dimension of intellect that remains almost unexplored, and, this being so, it is hardly possible to claim finality, or even approximate finality, for our intellectual map.

(ii) *Conceptualization*

As the intellectual skills of man evolve from lowlier forms of learning we note, first, the growth of temporal integration, which permits the diminution of repetition in learning. Secondly, we note an increasing power of relevant generalization. Generalization involves the ordering or classifying of experience. Concept formation or conceptualization is its simplest and most characteristic outcome. This process, like temporal integration, is of a very fundamental nature. It has been regarded by Hebb as the basis of learning (7).² It is certainly the foundation of naming and the use of language, and hence all the higher symbolic activities of man.

Oddly enough, except in a few badly constructed and imperfectly standardized tests, such as the Vigotsky blocks used by Hanfmann & Kasanin (6) and the Goldstein-Scheerer sorting test (4), tests which have been used primarily for clinical purposes, no adequate tests of the process of concept formation have been used for testing intellectual functioning. For we must distinguish with Rapaport (19) between verbal concept tests, such as the Bellvue similarities, and the active process of novel conceptualization. The reason for this lack would seem to be that test constructors have tacitly accepted the theory that intelligence consists of the grasping of relationships. This theory is most clearly enunciated by Spearman (22), who affirmed that 'everything intellectual can be reduced to some special case or other of educing either relations or correlates'. This theory derives from the same elementarism which reduced all perceptual figures to simple sensory units and relations between them, and overlooks the fact that in any structure whether perceptual or conceptual there is a principle of order, or what Kant termed a rule (14), which is not itself a relation or a mere sum of relations. Spearman himself endeavoured to explain the fact of structure in terms of his relation of constitution: the parts and their relations taken together constitute the whole. But this is either a tautology or untrue. It is a tautology if the principle of order is included as one of the elements; it is untrue as an equation if the principle of order is omitted. The strange relation of constitution is, in fact, a pseudo-relation; it is not a relation at all, but an illegitimate substitute for a principle of order. Involved in the formation of concepts is not merely the eduction of relations of similarity and dissimilarity, but the grasping of principles of order. It is to this, probably, that Piaget (18) is referring when he points out that 'a class presupposes a classification and independently of a general classification a generic term does not signify a class but an intuitive collection'.

¹ Humphrey (10) is an exception, and his theory of learning as four-dimensional integration has something in common with the views here stated.

² Loc. cit. p. 116.

Into this process of concept formation we may suspect that a type of abstraction and integration enters which comes only incidentally into conventional tests of intelligence. These tests involve for the most part simple relation and correlate eduction, not the ordering of manifold and heterogeneous experience through the grasp of abstract principles of order. Thus in Raven's *Progressive Matrices* (20), there is one and only one possible answer to each problem. The matrix presents a *determinate* system of relations, one term of which is missing. This is quite different from a *determinable* principle of order which can be variously exemplified. The matrix is relationally concrete in comparison with the concept. There is no evidence that the intellectual skill involved in concept formation reduces to any of the already discovered major intellectual factors. In the opinion of Humphrey (11): 'that the ability to generalize is identical with that measured by intelligence tests is in the highest degree improbable'. The matter anyhow can be settled only empirically, and at present the evidence is lacking.¹

The primary need if this path is to be explored is for the development of adequate concept formation tests. These tests, I suspect, should involve the time dimension, since the manifold which is conceptually ordered is a temporal manifold, and instances in real life are always temporally distributed. The child is not taken to Crufts before forming the concept of 'dog'. Tests of concept formation involving the time dimension can readily be contrived. Indeed in the experimental investigations of Aveling (2), Hull (9), Smoke (21), and Heidbreder (8), examples are already to hand, although in these investigations the materials were not used as tests.

Exploration of this particular path may prove of considerable importance. Spearman's theory of intelligence assumes a '*g*' which varies only quantitatively. All intellectual functioning from the perceptual to the most abstract levels is, according to Spearman, noegenetic and qualitatively identical. His theory is a one level theory; nor does Burt's hierarchical scheme allow for levels within '*g*' itself. On this view it is hard to give meaning to the findings of genetic psychology and psychopathology which suggest the importance of levels of functioning. If, however, intelligence involves not only the eductive laws of noegenesis but the apprehension of principles of order, the possibility of levels of intellectual functioning emerges, since these principles are themselves probably arranged in a hierarchy of levels. How far this concept of intellectual levels can be co-ordinated with quantitative differences of '*g*' is largely unknown. Here once more we find a tract of territory mainly unexplored.

(iii) *Judgement*

Finally, if intellectual skills are learned skills in which repetition is minimized and relevant generalization maximized the employment of the intellect should lead to progressive sophistication. Assimilation takes place, and wider, richer schemata of organized experience are built up. A quality something like wisdom accrues with the passage of time from intellectual exercise. This quality reveals itself in the process I propose to term 'judgement'. It is a process wherein the complex situations of the present are evaluated and assessed with reference to the assimilated experience of the past. The essence of good judgement is the appreciation of significance or relevance. The details of a situation too complex to be analysed are somehow structured according to their significance. Thus the local inhabitant steeped in weatherlore can judge sometimes better than a meteorologist

¹ Vinacke (28) notes 'the apparently low correlation' between conceptual tests and tests of intelligence.

whether, in a particular locality, it is going to be a fine day; or the skilled clinician can pick out just those tiny but significant signs which enable him to make a correct diagnosis. This process of judgement, whatever it is, is one of the most important of all intellectual processes in our adaptation to a complex, variable environment. The influence of past experience on present functioning has recently become amply recognized in the field of perception (see Hebb (7)), and was once amply recognized in the intellectual field in the Herbartian concept of 'apperception'. But it has played little part in psychometrics, and conventional intelligence tests endeavour rather to exclude, though with somewhat imperfect success, the element of sophistication.

Once tests are given serially, however, it becomes possible to reproduce the judgement process not merely in its results but in its formation. Artificial schemata can be instilled, and subsequently complex situations can be presented for assessment and evaluation in relation to those schemata. Is there over and above the experience upon which judgement is based a general capacity to acquire judgement? If so, is this capacity just 'g' or a broad group factor involved in all judgement situations? Or is it entirely specific to kinds of experience and situation and perhaps dependent as much on interests as on anything intellectual? The data to answer these questions do not exist, and our conclusion can only be that here again is a large tract of unexplored intellectual territory.

V. CONCLUSION

Briefly, what is the upshot of all this? The answer to our opening question must clearly be 'No'. Our map of the intellect is almost certainly not complete. Whole regions of the intellect would appear to be virtually unexplored. Some of these areas would seem to be capable of exploration by the test method, once the time dimension is introduced into tests.

The need for a theory of intellect, albeit only a provisional one, has been apparent throughout our discussion. Testing becomes a technology that gets lost in irrelevancies without theory to direct it. Theory provides the plans of the expeditions that strike into the unknown. Without such plans these expeditions are aimless wanderings. It has been suggested that in material from genetic psychology, psychopathology and learning theory the groundwork of a theory of intellect is perhaps beginning to emerge. The paths for exploration that have been suggested in this paper derive from a tentative theory. They are speculative, but neither wild nor untestable. They constitute the plans of possible expeditions, which indeed in a small scale are already on the trail.

The task of the psychologist remains as yet above all one of exploration, nor must he allow the elaboration of his equipment (manuals galore and statistics by the ton) to prevent him, by its encumbrance, from advancing from his settled holdings to the unknown hinterlands amid which these holdings are still precariously placed. For probably the unknown in psychology still vastly exceeds the known.

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THE TRANSMISSION OF KNOWLEDGE¹

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I. INTRODUCTION

In his Preface to the 1946 edition of W. K. Clifford's *The Common Sense of the Exact Sciences*⁽¹⁾, Bertrand Russell says: 'As the total amount of human knowledge increases and the journey from childish ignorance to the frontier of discovery lengthens, it becomes more and more important to hasten the process and to make the journey as easy as possible.' This implies four propositions concerning human knowledge: that there is such an entity; that it is growing; that it is important, and that its transmission can be accelerated. We may accept these in spite of the inevitable vagueness, pointed out by Russell⁽¹⁰⁾, in the very conception of 'knowledge'.

The sense in which I propose to use the term 'knowledge' is not an exclusively psychological sense. There is a sense in which knowledge resides in books or museums as well as in minds. We speak of the 'transmission of knowledge' implying that some entity preserves its identity through a succession of different carriers. The final carrier is the 'mind' or, if you prefer it, the nervous system, of a living organism. The process of *entry* into the mind was given the name 'Noëgenesis' by Spearman⁽¹³⁾. The present inquiry originated, in part, in a belief that the noëgenetic stages could not be adequately studied without an examination of the earlier extra-mental stages, i.e. the media of communication. For it is in these that knowledge is shaped. But it does not originate in the media. Confining ourselves for the moment to factual knowledge we seek its origin in facts. For knowledge gives its possessor an intimacy with fact.

II. THE NEED FOR 'DESIGNED' EXPERIMENT AND A CLASSIFICATORY FRAMEWORK

We know a good deal about atoms and molecules, about stars and planets, about plants, animals and men, about buildings, tools, books and pictures, about social and philosophical systems, about teaching and publishing and photographing and broadcasting, about talking and reading and writing, about seeing, hearing, learning and thinking. These are the various groups of more or less separate data in which the problem of the transmission of knowledge lies latent and fragmentary. The first task here is to interpret these scattered data so as to find what each group has to contribute to the solution of the problem, viz. that of formulating a systematic theory of knowledge-transmission. The solution will be brought about, as in all scientific problems, through the interaction of theory and experiment. What is not always appreciated is that experiment is not a mere process of accumulating unselected data. No one experiments without a policy and that

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policy originates in a theoretical conception. Further, the mere mass of data is not, as a rule, the most important aspect of an experiment. A single small observation (such as the shift of stellar images on a photograph of an eclipse by which the Relativity hypothesis was tested) may be of more moment than volumes of unrelated research findings. Thus the *design* of the experiment and its relation to the theoretical conception, are the criteria by which to judge its value.

Now purely *a priori* theorizing is rightly frowned upon in scientific circles. The kind of theory which bears fruit often originates in a tidying-up process applied to existing data, whereby a clearer view of the inter-relationships of those data is obtained. The theory then suggests that hitherto unsuspected relations hold between the data, and it proceeds to predict further data satisfying these relations. A new experiment is then designed to reveal these predicted data, or to reject the theory.

In this process the tidying-up stage is of considerable importance. It is essentially a process of deciding what goes with what, and what is different from what. In other words, it is a classificatory activity. The researches of scientists always take place within classificatory frameworks. We have only to recall the Linnean classification of living organisms or the Periodic classification of the chemical elements, and how the one led eventually to the theory of evolution and the other to the whole theory of atomic structure, to appreciate the significance of classificatory frameworks.

We therefore need a classificatory framework in which to accommodate all the data which contribute to the process of knowledge-transmission. The fact that knowledge can be of *anything whatever* (provided it is knowable) presents an initial difficulty, since it would seem to demand that we have to classify everything in the universe. In a sense this is true but it is not so impracticable as it sounds. The Periodic classification of elements applies throughout the universe, but it did not demand a complete knowledge of the universe. It provided a system for classifying one aspect of matter, viz. its chemical properties. Even elements unknown in Mendeléef's day were subsequently found to fit into the system.

III. THE STUDY OF MEDIATED IN CONTRAST TO IMMEDIATE KNOWLEDGE

What we are concerned with here are all those sequences of processes which lead to noëgenetic activity. Now *some* noëgenetic activity is the direct outcome of a contact between a fact and a person. Generally speaking this is the type of problem with which the philosophic theory of knowledge, i.e. Epistemology, is concerned. To judge by their writings one would suppose that epistemologists inhabit a world consisting of oranges and tables. But getting to know a simple object is the beginning rather than the whole of the knowing-process. It is extremely important in infancy and becomes progressively less important as language, education and techniques of distant communication steadily extend the range of the knowing process from the immediate environment to the distant nebulae, to remote epochs of time, to sub-microscopic physical structures and to the inner recesses of the mind itself. Clearly we have many modes of knowing and an adequate classification must embrace them all. The subtle difficulties discovered by the epistemologist in his *immediate* contemplation of an orange are but child's play compared with the complexities of the great bulk of our *mediate* knowledge of the universe. Putting the matter simply, the greater part of what a man *knows* consists of what he has read or what he has been taught by other people. His fragment of direct individual experience

is amplified beyond measure by the wealth of accumulated and recorded human knowledge, much of it centuries-old. Further, most of the practical problems of knowledge transmission, notably in education, arise precisely in connexion with the need for this mediated knowledge to be established in the mind of the learner in a limited time and with limited resources. I would suggest that not only the epistemologist with his immediate contemplation of the orange, but also the experimental psychologist with his study of immediate laboratory stimuli, would greatly extend their spheres both of theory and of practical utility by turning their attention to the far more general problem of *mediated knowledge*. Many researches suggest a failure to distinguish between the two, by treating the final stimuli of lengthy mediation processes as on the same footing as ordinary physical laboratory stimuli. There are two issues involved here. The first is the significant form of the stimuli themselves in relation to their antecedents in the transmission process. The second is the mental set induced by the background of previous knowledge of the learner in the particular field in question. The very word 'stimulus', with its psycho-physical history, can be misleading in this context.

IV. FOUR STAGES IN THE TRANSMISSION PROCESS

In order to accommodate the full extent of the transmission process I shall demarcate four distinct stages which may be briefly indicated by the terms *facts*, *records*, *communications* and *noëgenesis*. I must point out that the last three are also facts, albeit of specialized types. It follows that an adequate classification of facts would of itself accommodate the records, the communications and the noëgenesis. Moreover, we must bear in mind that knowledge in its fullest sense often includes *knowledge about knowledge*. For example, it is highly important for the military tactician to have knowledge about the extent of his opponent's knowledge of the distribution of the men and weapons in the battle area. Thus *knowledge* is one of the *facts* which can enter into the transmission of knowledge, and not merely as an epistemological subtlety but as a daily practical occurrence.

Some terminology is now required. I shall refer to *the study of the structure and transmission of knowledge* as 'Epistemics'. The continuing entity which is transmitted will be called an 'Epistemic Structure'. The four stages of the process will be called 'Epistemic Fields', separately indicated thus:

- (1) *F*-field—i.e. the particular area of *fact* involved in any particular transmission process.
- (2) *K*-field—i.e. the knowledge-record as expressed by the scientist or scholar who investigated the fact.
- (3) *C*-field—i.e. the activities and materials involved in the communication (e.g. in a lesson or lecture).
- (4) *N*-field—i.e. the noëgenetic processes by which the epistemic structure is established in the mind of the learner or recipient.

These four fields are all complex and are distinguished not so much by their nature as by their rôles in the transmission process. Each is, in a sense, a self-sufficient totality, having its own range of properties, but each has, in common with the other three, the epistemic structure which is the entity being transmitted. In virtue of this, and in spite of all the differences between the fields, there is a *projective correspondence* running through all four, for which reason we can speak of them as having some degree of *logical similarity*.

A full understanding of the transmission process will require a study of the separate properties of these four fields and also of the relations between them. We can express these relations thus:

$$(F) \rightarrow {}_fR_k \rightarrow (K) \rightarrow {}_kR_c \rightarrow (C) \rightarrow {}_cR_n \rightarrow (N).$$

${}_fR_k$ means the investigatory and linguistic processes by which the scientist or scholar abstracts the particular aspect of fact and gives it the written or other expression indicated by (K) ;

${}_kR_c$ means the interpretation and selection by which the communicator or teacher transmutes the content of (K) into a communicative activity, (C) ;

${}_cR_n$ means the psycho-physical relations and processes by which the stimuli from (C) give rise to noëgenetic activities (N) in the mind of the recipient.

Now is this division of the transmission process into four fields and three inter-field relations anything more than a mere tidying-up process? I suggest that it is. A good deal of attention is paid to-day, thanks to R. A. Fisher⁽²⁾, to the design of experiments. But I suggest that experiments need *strategy* as well as design. An experiment bears a relation to the advancement of understanding similar to the relation between a battle and a war. The tactics, i.e. the design of the experiment, are determined by statistical and methodological techniques. But these do not decide why the experiment should be carried out at all. Nor do they formulate the objective, i.e. the question which the experiment is intended to answer. These are matters of strategy. And strategy is determined by the theoretical framework which gives point and meaning to all the experiments carried out in any particular field of inquiry.

So far the framework simply shows four fields and three relations. We must examine the content and structure of the fields and the nature of the three relations, for otherwise we cannot understand the transmission process. To make the Epistemic Theory explicit at this point I will express the argument thus:

Noëgenetic activity cannot be explained adequately in terms of cognitive capacities and immediate stimuli alone. It must be regarded as the fourth of a sequence of related fields all having a certain abstract structure in common but each having also its own characteristic properties.

Now I believe this to be true of all genuine knowledge-transmission processes whether submitted to epistemic analysis or not. In other words the fact, the record, the lesson and the mental product are all structurally similar whether or not the structure is made explicit. But there are two good reasons why the structure *should* be made explicit. The first is the practical reason that the explicit generalization of structure facilitates *transfer of training*⁽³⁾. In other words, it maximizes the mental gain from the transmission of knowledge by giving the learner the power to apply it. The second is the theoretical reason that the first step in understanding the transmission process is the explicit formulation of the entity which is transmitted.

V. EPISTEMIC STRUCTURE

We must therefore consider epistemic structure, first in the abstract, and secondly as embodied in each of the four fields. In what terms are we to conceive and describe this abstract structure? If we put it in verbal terms this is tantamount to reproducing the Knowledge-Record which is only a particular embodiment of the structure. Obviously

we cannot handle the problem at all unless we have a theory of structure and a symbolic language in which to express it. And the theory must be such as to allow for the structure to be capable of four distinct embodiments. Spearman expressed his noëgenetic structure in terms of networks of *items* and *relations*. Whether or not this is adequate for noëgenetic structure you will find, if you attempt it, that it is quite inadequate for the other three fields. Wittgenstein⁽¹⁸⁾ declared that logical structure could only be *shown*, never expressed. But negative principles are not very convincing. If we allowed ourselves to be inhibited by all the negatives of the Logical Positivists we should end by admitting that we knew nothing at all. And whilst this would certainly be a final solution of the problem of knowledge-transmission we should not even know that it was and should be left still wondering.

If we turn to the *Principia Mathematica*⁽¹⁷⁾ we find that it consists very largely of logical structures. These structures are embodied in propositions expressed in a symbolic language. If sufficiently analysed down to their atomic propositions these molecular propositions would appear as expressing the structures of wide areas of complex fact. What they do not express, but only *show*, is their own structure. But the all-important point is that we only have to take one of these propositions and treat it as a fact in itself, analyse it and express its structure in another symbolic proposition. If the structure of a fact can be expressed in any language then the structure of the language can be expressed simply by treating the language as a fact. What we have to be clear about is *which* structure we are seeking to express.

This is not to say that the determination of structure is a simple matter. We have to decide what are the ingredients which constitute structure. We may examine the process by which the structures of the *Principia Mathematica* are built up. It starts from the declaration that: 'The universe consists of objects having various qualities and standing in various relations. Some of the objects which occur in the universe are complex. When an object is complex it consists of inter-related parts.' The statement goes on to assert that these complexes can be *perceived* and the occurrence of the relations can be *judged*. On this slender physical and psychological basis Whitehead & Russell erected the whole vast superstructure of their system, by introducing a number of building principles in the form of simple logical notions and primitive propositions. Given such a universe and so long as no logical errors crept in, the system is true. But is it *our* universe? It seems a very static affair. The only two *events* recognized in it are perception and judgement. The objects simply *stand*. They do not seem to move, to grow, or to have feelings. Of course it could be answered that one of the relations is the time-relation and that the translation of this universe into the universe of our experience is a matter of interpretation with which an extensional logic is not concerned. But in that case it is not concerned with objects, qualities, perceptions and judgements, though it *is* concerned with relations. (As a matter of fact perception and judgement were introduced to give a meaning to *truth*.) But if the system undertakes to provide meanings it should surely provide more adequate meanings. We can study structures in themselves simply for their own beauty, but as soon as we wish to identify them factually we must find *meanings*. If we wish to express the structure of *any* kind of knowledge, we must provide an adequate basis of interpretation for whatever logic we use.

This basis of interpretation, combined with a symbolic language, must enable us both to express whatever structures occur and to show how these are embodied in each of our four

fields. Without this we cannot have an adequate theoretical framework for the problem of knowledge-transmission, and cannot therefore begin to determine our strategy of experiment. Without it we are condemned to perpetual unplanned guerilla warfare in four unrelated fields. I see no escape from this need. The mind is not a closed system which can be studied without reference to external events. For that matter even an inanimate object is in some measure reactive to outside influences, but the mind has a far wider receptivity.

Epistemics as here understood is an independent study in the sense that though its roots lie in semantics, psychology, philosophy and even biology, it cannot be neatly subsumed under any of these. It comes nearest to what Prof. Gilbert Ryle⁽¹¹⁾ has recently hinted at in the following words: 'As there do exist the practice and profession of teaching, there could exist a branch of philosophical theory concerned with the concepts of learning, teaching and examining. This might be called "the philosophy of learning", "the methodology of education", or, more grandly, "the Grammar of Pedagogy". This would be the theory of knowledge in the sense of the theory of getting to know. This study would be concerned with the terms in which certain episodes in the lives of individuals are described and prescribed for by teachers and examiners.'

The theory of 'getting to know' sounds not very different from the theory of the 'transmission of knowledge'. Where Epistemics can claim some independence of conception and development is in its systematic study of the details of the whole chain of transmission, and in a determined attempt to shape its theory both to accommodate the data of experimental psychology and to yield definite guidance for the practice of education. In the handling of details it draws on whatever branches of knowledge are relevant.

VI. EPISTEMIC TERMINOLOGY

(i) *General*

To readers unfamiliar with Epistemics and therefore possibly disturbed by an apparently barbarous outbreak of neologisms it seems justifiable to quote R. H. Thouless's reference⁽¹⁴⁾ to 'the erroneous idea that first we have words with pre-determined meanings and our task is to find psychological facts to fit them'. 'The truth', he said, 'is the opposite: we start with experimental or observed facts and relationships between them. Our task is to devise a language by means of which to describe and co-ordinate these facts and relationships.' Dr Thouless went on to point out that there is no merit in the use of popular terms if they are infected with the vagueness of popular meanings. On the other hand, the mere invention of new terms is not of itself a contribution to theory. Only when a distinctive new concept is evolved should a new word be coined to express it.

Relatively few specifically psychological terms are used in the Epistemic System. The reason for this is that the system does not recognize what Whitehead⁽¹⁶⁾ called 'the Cartesian bifurcation' of mind and matter, and so uses terms which are as far as possible equally applicable in both mental and physical references, such as 'Event', 'Organization', 'Residue', 'Activity'. The need for this arises from the concept of knowledge as a structural entity passing through four different stages and therefore demanding continuity of terminology in order to express its persistent identity.

Epistemics adopts the term 'occurrent' as the most general word in its language. It is a sort of universal pronoun but it is pro-verb, pro-adjective and pro- any other part of

speech. Every separate item in the system, simple or complex, is an occurrent. The basis of the grammar of Epistemics lies in its treatment of every occurrent as having three dimensions. Ordinary linguistic grammar divides words on a one-dimensional basis according to their function in the sentence. It makes slight incursions into a second dimension in dividing nouns into concrete, proper, collective and abstract but does not apply this to other parts of speech. And it deliberately ignores the semantic functions of words.

Epistemics seeks to express every occurrent in a three-dimensional formula showing: (1) character, (2) function, (3) degree. 'Character' shows what *kind* of a thing, process, etc., the occurrent is. This will be discussed presently at some length. 'Function' shows the rôle of the occurrent in relation to other occurrents, as a logical 'part of speech'. A detailed account of the nine functions which constitute the grammar of Epistemics would be somewhat technical. They are adaptations of the basic concepts of the *Principia Mathematica* such as 'individual', 'class', 'variable', 'relation', etc. By expressing these in graphic symbols the epistemic language enables the structure of any piece of knowledge to be given a visual representation⁽⁸⁾. 'Degree' is an application of Russell's theory of Logical Types⁽¹⁷⁾. It represents the number of stages of abstraction involved in the occurrent. For example, 'a man' is a zero degree occurrent. 'The French' is a *class* of men and so a first degree occurrent. 'The nations of the world' is a *class of classes* and so a second degree occurrent. Similarly, we can have relations between relations, propositions about propositions, etc. Degree can be homogeneous, as in the foregoing, or heterogeneous, e.g. a relation between classes. The *object* of knowledge, whatever its nature, is always taken to be of zero degree. In any epistemic process there is always a cognizing mind and something cognized. The latter may be a physical object or event or something in another mind—it is still *objective* in our sense, and thus unambiguously of zero degree.

The notion of 'character' plays an important part in Epistemics. It attempts to provide a workable theory of universals. What is wrong with the Platonic heaven is the grammar of its eternal forms. In a language in which adjectives and verbs can masquerade as nouns, anomalies and contradictions are bound to occur. Similar contradictions occur in Mathematics if transfinite numbers are expected to behave like finite numbers. They do not. For example in the transfinite realm the whole is not greater than the part. In the physical world a body cannot be in two places at once. But a colour can be in many places at once. If 'body' is parsed as a noun we cannot in the same sense parse 'colour' as a noun. In psychology we are still suffering from the confusion engendered by parsing 'Intelligence' as a noun.

If by 'noun' we refer to inherently static unchanging 'substance', then there are no nouns in Epistemics, for in fact we never find such a thing in the universe. As Whitehead has shown, we always find 'events'⁽¹⁵⁾. 'Character' is what distinguishes one event from another, apart from structural and external criteria. Every event is unique, but two or more events may manifest the same character. A series of events having the same character constitute a 'property'. Properties are always observed to occur in groups, and the locus of this occurrence is what we call an 'object'. In Epistemics this is called an 'organity' to escape the static associations of the word 'object' and to express the notion of an *organized unity*. There are 'primary organities' and 'derivative organities'. This is one of the most distinctive concepts of Epistemics, and merits some elaboration.

(ii) *Primary and derivative organities*

When we study our environment, containing the sources of all our external knowledge, it is at first a highly complex and heterogeneous field of interacting objects with many different properties—men, mice, mountains, moons and the like. If we could see them in a time-perspective we should observe two facts. First, the objects sort themselves out into groups according to the epochs at which they first made their appearance in the universe, i.e. there is an evolutionary scale; secondly, within each group there are certain primary objects which appeared first and which, by their interaction with each other and with objects of earlier groups, by combination, reorganization or fragmentation, produce whole crops of *derivative* objects. We find that the kinds of reasoning appropriate in dealing with primary objects goes astray when applied to derivative objects, i.e. it is *non-transferable*.

The characteristic features of a primary organity are these: (1) it is a system of 'events'; (2) it has a boundary; (3) it has a centre or nucleus in which organizing forces originate and maintain the organity as an enduring structure. Atoms, stars, animals and human beings are examples of primary organities.

Derivative organities are also bounded systems of events, but either they lack a nucleus or their organization is not entirely determined by the nucleus. Clouds, rocks, shells, nests, machines, buildings, books are examples of derivative organities.

The organities are the striking and obvious features of our environment. Because their properties are sequences of recurrent 'events' we build up expectations about them, and it is the fulfilment of these expectations which leads us to regard them as static and unchanging. They provide the concrete nouns of our language and the certainties of our emotional needs. For this reason the other parts of speech are festooned about the nouns and we cannot use a verb without a 'subject'. Thus, although events are the stuff of actuality, *nouns* are the stuff of language. It happens then that we cannot *describe* events without using nouns, in spite of the fact that the nouns themselves refer to systems of events. There is a regress here which can be solved if at all only by metaphysics. But it is avoided in practical Epistemics by the Principle of Topic Limitation (which is an extension of the concept of the 'Universe of Discourse' used by the logicians):

'In every limited area of knowledge consistent treatment can be attained by confining every Topic to *relations lying between an outer boundary and an inner boundary*. The outer boundary is the limit of the topic. The inner boundary is the set of unanalysed nouns used as the terms between which the relations hold' (8).

For example, in studying the structure of an organism we should *not* seek to analyse its tissues into their molecules. In studying a tissue we should *not* seek to analyse the molecules into their atoms. In studying molecules we should *not* seek to analyse their atoms into electrons and nuclei.

It is impossible to deal fully with organities without also taking account of the surrounding medium. This is an undefined term in the Epistemic system. As the nineteenth-century history of the Aether-theory shows, the language for discussing a medium does not follow the same grammar as that for the objects in it. It has no boundary and no organization. Yet it transmits events and is a carrier of relations. Through the latter the medium yields the concept of space. Similarly, relations between events yield the concept of time.

(iii) *Events and relations*

Events are divided into three groups: (1) external events, i.e. between organities; (2) surface events, i.e. changes in surface properties of organities; (3) internal events, i.e. between the components of organities. In describing events we may use verbs such as *walk, fight, eat, build*, whose meanings are inherently complex. To analyse an event we ask: what *changes* does it involve? And we find that it always involves changes in the surface properties of the objects at the lower boundary. In fighting, for example, there are changes in the positions of the limbs, cuts, bruises, etc. on the skin, rise of temperature and so on. This analysis points a way to a systematic treatment of relations (5, 9).

It is not easy anywhere to find a satisfactory account of the meaning of this ubiquitous word 'relation'. It is essentially an abstraction. But what is it abstracted *from*? There was a certain value in Spearman's division of Relations into 'real' and 'ideal', but his language was misleading and he worked on static assumptions (4, 5). Events in the mind are as real as any outside. But there is a difference in the relations they yield. In Epistemics a relation is defined as 'a cognized abstraction from an event involving two or more terms'. The terms may be organities, or sub-organizations within a single organity. An idea, or any other psychological entity, is a sub-organization within an organity. When an event occurs it is 'full' in the sense that, however simple it is, the participating terms are fully engaged with all their properties. Bending a little finger involves reactions all over the body. A relation is a selection of *one aspect* of the event and concerns a single property or a narrow range of properties. It is *real* in the sense that this aspect is certainly present, but it is abstract in that all the other aspects are ignored.

Consider now three different relations:

(1) The relation of *seller to buyer*. This is an aspect of an event involving four terms, a transaction in which a commodity passes from seller to buyer and money passes from buyer to seller. All relations thus arising directly out of events will be called *direct relations*.

(2) The relation of *brother of*. Here there need be no event actually involving the two terms simultaneously. It is possible for two brothers to be so separated in time or space that they never meet. Yet they are brothers. The relation lies in *two past events*, viz. their births, both involving one and the same third term, viz. their mother. All relations of this type will be called *historical relations*.

(3) The relation *north of*. Here again there need be no event involving the two terms whether present or past. The relation arises in somebody's *mind* from a joint consideration of the positions of the two terms. These 'considerations' are mental sub-organizations and there is an event bringing them together. The relation arises out of this event. All relations of this type will be called *formal relations*.

Logic handles all three types of relation indiscriminately and elicits their logical properties of transitivity, symmetry, etc., but the *semantics* of these three types are obviously different and should influence our use of them in reasoning. Since relations are the most important elements in knowledge-structure a clear understanding of their nature is of the highest importance.

(iv) *Characters*

Returning now to the concept of 'character' the policy here is to proceed cautiously within the limits of our present understanding. It took mathematicians a long time to learn to handle transfinite numbers consistently, and we have not yet learnt how to handle universals, i.e. characters, without getting bogged down in the old Realist-Nominalist controversy. If we confine ourselves to the empirical evidence for characters we shall be safe, so long as we recognize that they must have a grammar of their own, even if we cannot yet formulate it. Our only *direct* acquaintance with characters is in the qualities of our own experience. Since experience is the matrix in which knowledge-structure is embedded, i.e. the final stage of the transmission process, these experiential characters are of special importance in Epistemics. They can be classified in various ways, but for convenience they are divided into seven groups. In any actual psychological event there may be characters from several groups participating but usually one or two are paramount. These seven groups are called Epistemic Status-Types. They provide us with seven varieties of knowledge, a considerable extension of the French distinction between *connaître* and *savoir*. They are as follows:

- (1) *C*-status—i.e. *Contact* experience originating in the skin.
- (2) *S*-status—i.e. *Somatic* experience originating in the internal receptors.
- (3) *P*-status—i.e. *Proprio-ceptive* experience originating in the muscles, tendons, joints and semi-circular canals.
- (4) *E*-status—i.e. *Emotive* experience originating in the thalamic region.
- (5) *O*-status—i.e. *Observational* experience originating in the distance receptors of vision, hearing and smell.
- (6) *N*-status—i.e. *Nominalational* experience originating in the vocal organs and speech centres.
- (7) *I*-status—i.e. *Ideational* experience originating in the cortex.

It seems clear that we can have ideational knowledge of events of which no direct perceptual knowledge is possible, e.g. events within the atom. Only philosophic idealists doubt the actuality of these events. In proportion to the adequacy of the ideation there will be a structural correspondence between the *I*-status knowledge and the occurrents themselves. It is our belief in such correspondence which leads us to feel some degree of confidence in our knowledge. The structure itself is a complex relationship of complex terms and the occurrent is *dense* in the sense that by abstracting different relations different structures are achieved. As a line is dense with points an occurrent is dense with structures. Thus knowledge is always selective and the cognitive structure of the mind is only a pale skeleton of the actuality of the universe. But it is because the structure which eventually reaches the mind was genuinely present in the actuality that we can claim to have a partial knowledge of the universe. The language of Epistemics enables these structures to be formulated in characters, functions and degrees at each stage of the transmission process and so enables us to design experiments which, by using a common language and by working within a common framework, can yield comparable data. From these data we can hope in time to determine the general laws governing the transmission of knowledge.

VII. STRUCTURE AND CHARACTER

So far, I have dealt not so much with the transmission of knowledge as the logical analysis of the elements involved in the transmission. In doing so I have had to take account of the fact that whatever kind of thing we analyse we find that the elements which the analysis reveals are themselves capable of further analysis. For this reason I prefer the term *occurrent* to the term 'element' which has an unwarranted suggestion of ultimate simplicity. Occurrents are dense with structures. Philosophers and poets seek to fathom the total density. Their activities give us imaginative insight and appreciation but not practical knowledge. The scientist works within self-ordained limits and studies prescribed structures only. Fortunately for him Nature lends itself also to this treatment. For though total structure is indefinitely complex it is *stratified* and *segregated* to a certain extent into partial structures of limited complexity. Hence the principle of Topic Limitation. Groups of characters can be segregated to yield finite studies.

My use of the term character includes the experienced qualities of psychological events, but it is not restricted to these. These are the characters of the final stage of the transmission process, but every stage is assumed to have its own characters. The difference between this last (psychological) stage and the other three is that we have direct acquaintance with its characters. The characters of the other three stages can be formulated only indirectly. But the formulation is none the less cogent for all that. To illustrate this, consider the distinction between our discrimination of *green* from *black* and our discrimination of *grass* from *coal*. The first is an immediate discrimination of two directly experienced qualities. The second is based on two whole series of experiences each with many qualities. It is true that we still make the discrimination by virtue of directly experienced qualities, but the grass and coal are taken to be independently different in their own right. This is because they have their own characters. We cannot ostensibly name those characters as we can name *green* and *black*. But we can indicate their occurrence by calling them grass-characters and coal-characters, i.e. by using the discrimination between our two series of experiences to apprehend a distinction independent of those experiences, a distinction between two independent organities. It is through such discriminations that the materials of the known universe are segregated and apprehended. Our knowledge consists of mental structures in which material objects are represented, in which their various properties can be separately distinguished and in which events involving them are also separately distinguished, though in the actuality there is no separation of these three aspects. Our mental activity also abstracts relations, groups the occurrents into classes, establishes symbols having semantic relations with occurrents, of first and higher degree, symbols for objects, symbols for properties, for multiplicities, for series and other structural arrangements. It matches one structure with another and so arrives at the relation of *correspondence* or *truth* or *validity* on which propositions and systems of propositions rest. The simple Noëgenetic scheme of fundamentals, characters, relations and correlates is true as far as it goes but it is a gross over-simplification. *And if we are to carry out experiments on knowledge-transmission we must have a means of formulating the knowledge-structures which result from the transmission.* Otherwise we cannot talk about the most important factor in the whole process. The Epistemic language aims at providing an adequate means of talking about structure.

I have tried to show how the conventional grammatical parts of speech—nouns, adjectives, verbs, etc.—enter into the problem, since these are the elements of our ordinary mode of communication. But each has misleading implications when applied, without analysis, to both mental and physical fields in the same discussion. I have therefore tried to provide a system of language-elements free from misleading implications. Each element has character, function, and degree. The specification of characters begs no questions for it is based on a classification of discriminations which we all make. The specification of functions is a refinement and generalization of the grammatical parts of speech in accordance with *logical* rather than conventional criteria.

VIII. CONCLUSION

Thus the knowledge-transmission process is exhibited as a dynamic structure consisting of four fields of analysable occurrents with intervening events carrying the structure of the initial field of fact through to the terminal field of mind via the two intermediary fields of record and communication, with one and the same system of analysis and formulation applicable throughout. This is the qualitative framework within which we can determine the strategy and design of our quantitative experiments. I have tried to avoid begging any questions. Whilst the system is primarily designed for studying the transmission of normal empirical knowledge it does not exclude the possibility of knowledge of spiritual values or of para-normal modes of cognition. It only asks that the data and concepts of such studies shall be clearly stated. It should then be possible to determine which of two alternatives is correct: either (a) the data and concepts will be found on analysis to be reducible to the same types of occurrents as those of empirical knowledge and thus expressible in the Epistemic Language; or (b) this will not be the case, and a different language-system will then be required.

An important principle in Epistemics is that *explicit particular knowledge always contains implicit general knowledge*. The behaviour of a particular man is an individual exemplification of the behaviour of men in general. The structure of a particular plant is an exemplification of the structure of plants in general. The properties of a particular star are an exemplification of the properties of stars in general.

But particular knowledge mostly remains particular and fails to yield up its potential implications. One aim of Epistemics is to indicate both the need and the procedure for achieving this. When a particular instance is seen as a case lying within a general framework then the full pattern of relationships within the framework is seen to belong to the particular instance. Our legal system rests on this principle. The function of the court is to discover in each particular misdemeanour or dispute the general implications so as to bring it within the framework of the law. This is possible only because the lawyers and justices know the framework. The individual litigant or offender thinks he can get away with it because he does not know the framework. Similarly, the reasoning of the individual within the field of particular knowledge is often inadequate or fallacious because he has never grasped the framework of total knowledge.

Epistemics therefore points to a fundamental educational requirement, viz. that in every case of transmission of particular knowledge the relations of the particulars to the general framework should be made manifest (6, 7). This will both reinforce the grasp of the particulars and help to develop the general framework. Through the latter it will facilitate the grasp of later particulars. A sort of self-inductive process is established.

Epistemics thus has two aspects: first in relation to present educational practices; and secondly in relation to an ideal of human cultural development. Present practices are sadly lacking in general principles and widely fail to achieve their objective which should be *integrated mental growth*. As psycho-analytic theory has provided a theoretical framework of personality-structure by which individual maladjustments can be interpreted, so the Epistemic Theory seeks to provide a theoretical framework by which maladjusted education can be interpreted. Epistemics does not prescribe teaching-methods. It analyses them. The method is always an expression of the man but what he does always has implications within a framework of which he is commonly unaware. By becoming aware of it he can achieve a better expression of his aims, and also a clearer definition of those aims.

This framework goes beyond the somewhat bookish conception of the curriculum so often assumed in educational discussions. The curriculum itself is an interpretation of the environment and history of man. Curricular knowledge perforce partakes, however implicitly, of both the biological and the philosophical issues which permeate man's life, and cannot be adequately viewed through the somewhat tinted and polarized spectacles of a narrow educational psychology. In particular, the bearing of evolution must be faced. We are apt to suppose that evolution is concerned with the past, or at any rate that only its slow, contemporary advance can affect us to-day. This is to ignore the fact that we live in an environment composed of the products of evolution, and of the results of the interaction of those products. Human knowledge, man's knowledge of his environment and of himself, is a knowledge of *evolved forms*. A theory of knowledge, to be adequate, should reckon with this highly significant circumstance. As Sir Charles Sherrington⁽¹²⁾ has said: 'There are words which some turn in the history of thought suddenly overwhelms with a large special meaning. Such a one is evolution.' It would be a pity if our attitude to evolution became so dusty with the ashes of nineteenth-century controversy that we lost sight of its continuing impact.

The evolutionary framework of Epistemics (or Model Universe, as it is called) takes as *limited* and *classifiable* the number of radically distinct types of objects in the universe; their primary properties; the varieties of primary human experience which form the psychological content of knowledge; and the fundamental logical forms embodied in language and determining the structure of knowledge. The unlimited variety of interactions and relations which constitute the dynamic environment and life of man can be given analytical expression in terms of this framework. It thus provides a strategical map for the planning of systematic experimental work on knowledge-transmission processes. It is my further hope that its analytical method may provide an instrument for surveying and more rationally exploiting the mountain of knowledge accumulated through man's investigatory activities, a mountain whose mass and complexity fill with dismay all those who wish to see its riches more widely spread.

IX. SUMMARY

This paper is an outline of the system of Epistemic Analysis. This may be regarded as a modification and extension of Spearman's system of Noëgenetic Principles. It discriminates more variables than those indicated by Spearman in the cognitive processes themselves. It also provides an objective structural and genetic analysis of the stimuli which initiate those processes. The experimental and statistical investigations of cognitive

performances presuppose a classificatory framework which enables the variables to be qualitatively interpreted. Commonly this framework is not made explicit and the variables of one experiment may be erroneously identified with those of another experiment tacitly assuming a different semantic framework. Only within a single framework is it legitimate to compare the results of different experiments. The Epistemic System offers a common framework for all researches on the transmission of knowledge, and a method for identifying the structural components of topics within the ordinary curricular branches of knowledge dealt with in education. It traces the transmission-process from the original factual structure, through the symbolic structure of knowledge-records and the educational structure of the teaching process, to the final noëgenetic structure which constitutes the cognition.

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THE INSTINCTIVE DISPOSITION TO ACT INTELLIGENTLY¹

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I. *Introduction* (p. 336). II. *Man's capabilities* (pp. 337-338). III. *Inferences from observed behaviour* (pp. 338-341). IV. *Mental tests as samples of conduct* (pp. 341-344). V. *Inferences from test data* (p. 344). *References* (p. 344).

I. INTRODUCTION

The problem

People attempt to measure intelligence and draw inferences from the results of mental tests without any clear idea as to the nature of mental activity. In this paper, which, for want of a better title, I have called 'The instinctive disposition to act intelligently', I wish to show that instincts and intelligence have more common qualities than distinguishing attributes, and that some more exact psychological account of mental activity seems necessary.

To this end I propose to ask two fundamental and two subsidiary questions, namely:

(1) What are man's chief capabilities and how can we best describe them?

(2) What can we infer about people from the ways they behave?

(3) What is a mental test and what do tests record?

(4) What inferences can be drawn from test data concerning a person's past, present and future?

I shall deal chiefly with the first two questions, and discuss what I shall call the 'presentational and sequential laws of conduct'.

A mental test is simply a sample of a person's conduct at a particular time and under given conditions, from which we can infer group similarities and individual differences of past mental development, and make predictions concerning future conduct which can be verified by observation.

Converging tendencies in psychology

The past generation of psychologists tended to describe human nature from two distinct points of view. Academic psychologists, approaching the subject chiefly from philosophy or education, interested themselves mainly in intellectual activity and regarded intelligence as the endowment whereby man acquired knowledge and acted with understanding and foresight. Medical psychologists, approaching the subject chiefly from biology and the social sciences, interested themselves mainly in human behaviour and endowed the instincts with the qualities whereby even man's unconscious actions strove towards the achievement of future objectives.

At the present time philosophers and educationalists tend to consider the instinctive basis of conduct, while medical psychologists recognize the importance of intellectual activity in determining the effectiveness with which people achieve their objectives. Half a century of disputation as to the nature and inheritance of intelligence, the number of human instincts and the innate forms in which they manifest themselves suggests, however, that instinct and intelligence cannot be clearly differentiated one from the other.

¹ A paper read at a meeting of the British Psychological Society on 19 May 1951.

II. MAN'S CAPABILITIES

Whatever our approach may be, as soon as we begin to observe other people we notice that their behaviour seems to some extent a repetition of previous behaviour and at the same time a progressive modification or development of it. The popular tendency is to think of this repetitive and progressive development of conduct as the expression of instincts and intelligence. The trouble is that the words 'instinct' and 'intelligence' have so many shades of meaning that they are useless for the purpose of exact description, while they encourage us to believe that by giving a quality of conduct a name, we have also explained it.

Conduct is apt to be regarded as 'instinctive' if it is carried out easily and successfully with little or no deliberate thought. If difficulties give rise to rational judgement and deliberate action, the ensuing conduct tends to be regarded as 'intelligent'. Amongst people who use the words in this way there tends, however, to be a shift of emphasis from the quality of the conscious activity involved to the quality of the result obtained. Instinctive actions come to be regarded as blind, often inappropriate, driving forces, while it is assumed that by his intelligence man frees himself from his instinctive impulses. More frequently the distinction appears to be between the tendency people show to act habitually in particular ways and their tendency to modify their conduct to meet new situations, but here again there ensues a shift of emphasis from the repetitive and original qualities of conduct to the period in the development of the individual at which a mode of conduct is supposed to have been formed. Habits to which there appear to be inherited tendencies are regarded as instincts, while those which are created during the life of the individual, and are assumed to die with him, to be acquired again in succeeding generations, are regarded as expressions of intelligence.

The difference between instinctive and intelligent behaviour is never very clear. An instinct is said to be a 'force' and at the same time an 'inherited disposition', but in exactly the same way intelligence is said to be 'mental energy' and at the same time to necessitate acquired knowledge. When definitions are attempted, an *INSTINCT* is generally defined as *an inherited disposition to particular kinds of emotional excitement and forms of conduct in response to specific situations*; but one of man's inherited dispositions is his tendency to respond to situations more or less intelligently. In fact, *INTELLIGENCE* can only be defined at all satisfactorily as *an individual's innate disposition to modify his conduct in such a way that he continues to preserve his individuality in a changing environment*.¹ Such behaviour would seem to satisfy all the conditions for it to be regarded as a human instinct. It occurs quite spontaneously in situations of a particular kind, namely when an habitual response to one's environment gives rise to frustration, it is accompanied by the well-defined emotional excitement of anxiety during the period of frustration, and joy when a solution is found, while the resulting behaviour at once repeats the past and purposively pursues some future objective.²

¹ The only alternative seems to be to define 'intelligence' as a person's ability to do what his examiner thinks is right. This is well illustrated by some tests of intelligence used, for example, by R. B. Cattell, and by the opinions expressed by some teachers.

² It may be argued that while the instincts pursue separate ends, intelligence is at the disposal of all the instincts and contributes to the effectiveness with which they achieve their various ends; but those who make distinctions of this kind will also argue that intelligence only gives rise to specific rational judgements, the libido or energy of the sex instinct being the motivating force underlying all forms of intellectual activity.

If the words 'instinct' and 'intelligence' are only names for different qualities of a single activity, and not even clearly differentiated opposite qualities, current psychology needs revising. This can be done if we keep in mind two things. We must recognize that in psychology we are dealing with a continuous flow of change, or more exactly conative activity, and formulate our definitions accordingly. We must also avoid the behaviouristic, analytic and academic mistakes of describing psychological activity as being 'nothing but' the inevitable outcome of the particular explanatory principle we happen to be interested in. Instead of offering subjective, or more correctly speculative, interpretations of human conduct, we must consistently adopt the objective method of empirical science, or more correctly that of inference and verification by comparative study.

III. INFERENCES FROM OBSERVED BEHAVIOUR

The psychologist is interested in the things people do, and in doing, know. Knowledge can only be acquired as the result of acting. We have to attend to an event in order to know what is going on. Even when it is simply holding an object in our hand and looking at it, or keeping an idea in mind and contemplating it, what we do determines what we know. The psychologist attempts to compare the different ways in which people take part in events, the things they try to achieve and how as a result they organize their thoughts and actions. The degree to which he understands people, however, depends upon his understanding of his own experiences and the inferences he draws concerning the behaviour of other people. Without systematic introspection of the feelings, perceptions, memories, desires and decisions accompanying our own actions, we are unlikely to understand the determinants of human conduct.

Introspectively, the self can be said to be the origin of observation. Every statement we make can, for example, be prefixed by the phrase 'I observe that' e.g. 'I observe that I feel thirsty'. What we observe are, of course, our experiences: our feelings, thoughts and actions. In all our experiences, whether we are attempting to achieve something or trying to perceive some specific quality of an event, we are intentionally active. Our perception of events is *INTENTIONAL* in the sense that we always respond to and perceive events as if *reaching out with desire or effort towards the achievement of some objective* at the time not necessarily known or pursued with deliberation.

According to our intentions and expectations we actively select and only attend to certain qualities of events which we apprehend as forming a figure standing out from its ground, or more correctly as an individual entity existing in an environment. We perceive an individual entity in the sense that any thing we intentionally take hold of or, as we say, perceive, appears to us to possess a characteristic organization of its own and to be at the same time systematically related to its surroundings. Some of the things we observe appear inanimate; others appear to be living. One difference we notice is that living things preserve their individual existence in a changing environment; and the more a living individual appears to anticipate environmental changes and appropriately adapts its activities to them so that it consistently pursues its own objectives, the more we think of its behaviour as the outcome of mental activity.

From the behaviour of living individuals and from our introspective awareness of pursuing our own objectives, we interpret the behaviour we observe as the manifestation of conative activity, or of intentional striving towards certain ends. We can therefore

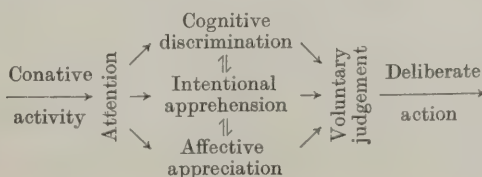
define CONATIVE ACTIVITY, or purposive behaviour, as *intentionally organized conduct inferred from the observed behaviour of living individuals and from the observer's introspective awareness of the intentions determining his own conduct*. CONDUCT in turn can be defined as *behaviour guided by experience*, while ATTENTION can be said to be *conative activity directed towards the cognitive discrimination and the affective appreciation of particular qualities of experience facilitating voluntary judgement and effective action*. From this point of view, PERCEPTION can be defined as *the intentional apprehension of one or more qualities of an experience according to the objective a person is attempting to achieve*.

By attending to our experience we apprehend, or take hold of, certain qualities of events with greater discrimination and personal appreciation and are better able to judge between them and so act effectively. Our intentions determine how we respond to an event, while our response determines what we think is the stimulus causing us to have certain perceptions or to act in particular ways. Again any objective we pursue, whether we perceive it as a 'thing' or think of it as an 'idea', 'purpose' or 'plan', always has some *meaning* for us as a result of our past experience and some *significance* for us according to our needs and intentions for the future. In other words, what we perceive is the outcome of an activity, the formal and somatic determinants of which always lie in the past, the final and psychic determinants of which lie in the future.

According to its past meaning and future significance for us, any action we take tends to be accompanied by at least some degree of both cognitive discrimination and affective appreciation. Some psychologists use the word 'affection' to mean simply feelings of pleasantness or unpleasantness accompanying an experience, but affection, as we are introspectively aware of it, is usually more than this. While COGNITION can be defined as *the essential discriminative component of conduct*, AFFECTION is probably best defined as *the essential appreciative component of conduct*; as a result of which a person accepts and in this sense participates in an event or rejects it and to this extent remains detached from it.

When an event affects us only in one way, our response is usually immediate and complete. Things usually affect us in more than one way. When this happens we tend to be aware of alternative and sometimes conflicting ways of responding to them. The result is that for a time we are unable to respond effectively. Instead, we have to ask ourselves what we want and to consider alternative forms of action and weigh their consequences. Deliberate or voluntary action occurs when, having considered alternatives, we decide to respond to an event in a way we believe will secure some desired end. Intellectual reasoning, far from being the ultimate criterion of what we do, simply discovers the relationship between things, and indicates ways of doing what we desire to do, or provides us with excuses for our actions.

Within the limitations of a two-dimensional diagram the differentiation of conative activity facilitating effective action can be illustrated as follows:



With these co-ordinates of conduct defined we can consider what may be called the 'presentational and sequential laws of conduct'. Jung speaks of 'simultaneity' in contrast to sequences. 'Coincidental' would be a convenient word to use were it not for the modern tendency to speak of coincidence disparagingly. The word 'presentational' has the advantage that it suggests something presented in the sense of being simultaneously present and coincidental with other items of experience. The word 'simultaneous' can be retained to mean coincidental but not necessarily presented here and now.

Spearman⁽³⁾ enunciated three presentational laws of experience, namely that: '*Any lived experience tends to evoke immediately a knowing of its direct attributes and its experimenter*'; that '*The presenting of any two or more characters tends to evoke immediately a knowing of a relation between them*'; while '*The presenting of any character together with a relation tends to evoke immediately a knowing of the correlative character*'.

In his effort to enunciate sequential laws, he was less successful, chiefly as a result of treating together temporal sequence and quantitative changes. Swayed by current views concerning the inheritance of intelligence, he began with the demonstrably incorrect statement that: 'Every mind tends to keep its total simultaneous cognitive output constant in quantity, however varying in quality'. He proceeded to state that: 'The occurrence of any cognitive event produces a tendency for it to occur afterwards', but that it also 'produces a tendency opposed to its occurring afterwards'. He went on to state that: 'The intensity of cognition can be controlled by conation', but that: 'Every manifestation of the four preceding quantitative principles is superimposed upon, as the ultimate basis, certain primordial but variable individual potencies'. Clearly these so-called 'quantitative principles' are inadequate attempts to state essential qualities of experience which Spearman was acutely aware of, but failed to define.

Although we tend to perceive things as static objects, and imagine them existing independently of time, cognition always involves change, and change, time. At the beginning of the century Titchener⁽⁴⁾, following James and Bergson, had pointed out that 'In the world of experience nothing stands still, whatever we observe and from whatever standpoint we observe it, we observe change'. He argued that mankind seeks to arrest this flux and to give stability to the world of experience by assuming permanent substances, but just as physicists have to give up the concept of unchanging matter, so psychologists have to give up the idea of unchanging mind.

The problem is not so much how we perceive change or 'movement' as how, and why, we imagine substantial unchanging objects and form ideas of living individuals persisting through time and possessing enduring qualities of character. Briefly the explanation seems to be that every response of a living individual to its environment re-enacts, and is at the same time a modification of previous responses. Whether we introspectively observe our own thoughts and behaviour or observe the behaviour of other people, this repetitive and progressive development can be described in the form of three simple statements.

The first is that *conduct maintains and repeats its forms of expression*. In other words, once an idea or way of acting is initiated, it tends to persist and to be repeated. It is from this tendency of thought and behaviour to maintain and repeat its acquired organization that we form our ideas of persistence, memory, habits, instincts, organic structures, and even of the enduring existence of matter.

The second is that *organizations of conduct which have been acquired provide the initial*

forms from which subsequent conduct develops. In other words, what we have done determines what we are able to do; the knowledge and skill we have already acquired provides the basis for acquiring further knowledge and skill and determines the point at which we must begin.

The third is that *organizations of conduct which are attended to and developed are determined by what a person is deliberately or unknowingly tending to achieve or become.* In other words, our intentions determine what we notice and recall; it is the future action we want to carry out or the idea we want to express which determines what we attend to in the present, and the particular habits, techniques or items of information we recall from the past. Except in morbid conditions, a person does not repeat habits or recall items of information indiscriminately. What he recalls, and how he acts, is determined by what he wants to achieve.

Because our intentions determine what we recall of the past and how we act in the present, and because people usually form some more or less clear plan of how they wish to act, the tendency is to describe living activity as having a purpose or achieving some end. The mistake is to assume that because we can intellectually conceive a purpose or plan to guide our actions, it also explains them. The more we delve into the past, the less we are able to conceive any original cause or beginning of what we now are. In the same way, the more we look into the future, the less we find any psychological explanation of the purposes we at present pursue. At the same time, the more we recall the past and apprehend the future, the more we are able to meet events with satisfaction, while the decisions we take in the present appear to be the cause of what we do, and also the end achieved. Just as we always appear to be emerging from the past, so the future appears to be coming towards us, the present consisting chiefly in adapting past forms of conduct to the needs of the future.

Man's chief needs are those of protection, feeding, excretion, sleeping, recreation, the fellowship of his kind, and a mate of the opposite sex. These are so essential to the welfare and happiness of the individual and the preservation of the race that their expressions through the ages have acquired highly developed organic systems and modes of conduct which largely determine a person's present response to particular situations, the emotions he experiences, his apprehension of events, and the way he behaves. They embody the acquired wisdom of the race, and are systems of conduct by which man is able to express whatever may be in him to be.

People resemble each other in their basic needs and modes of conduct. They differ chiefly in what can for the present be described as the location and extension (or range) of their awareness, in the direction and mobility of their responses and the way they organize their thought and action in the pursuit of their objectives. To compare people one with another we need to determine group similarities and individual differences, and variations with age and health in a person's present capacity for cognitive discrimination and affective appreciation, his span of apprehension, his speed of intellectual work, his trend of associations, and the way he organizes his thought and conduct.

IV. MENTAL TESTS AS SAMPLES OF CONDUCT

A mental test is a sample of a person's conduct at a particular time and under controlled conditions. The belief that a test will elicit a particular kind of conduct is simply a hypothesis which may or may not be verified by observation. The success with which an

investigator makes a critical judgement that a certain test will elicit conduct of particular interest depends upon the purpose he has in view and his psychological insight. The success with which he makes the comparative judgement that the conduct elicited resembles the conduct it is intended to sample, depends upon the technique he uses in order to compare different forms of conduct, and to determine the degree to which they are similar or different.

One of the most interesting qualities of human nature, and perhaps the most important is man's capacity to do things skilfully, whether it is in attaining what he wants to achieve, in organizing his knowledge, or in his relationships with other people. Skill is sometimes thought of simply as acquired knowledge or technique, as when we speak of a 'highly skilled' person meaning one who is highly trained. At other times the word 'skill' is used to refer to the aptness with which a person utilizes his knowledge. Both aspects of skilful work are important. According to the dictionary, SKILL is '*familiar knowledge of an art united with dexterity in the practice of it*'. Without specialized knowledge and technique, it is impossible for a person to act skilfully. At the same time a person who relies on the knowledge and technique he has acquired in the past may not act skilfully. A skilful person does not simply recall acquired information. He compares his knowledge of how things have been done in the past with situations as he observes them in the present, and the objectives he wishes to achieve. Even acquired knowledge and the intellectual capacity to form comparisons and reason by analogy are by themselves not sufficient. It is the aptness with which he acts with respect to his objectives that make his actions skilful.

The capacity to acquire skill is often spoken of as 'general intelligence'. Unfortunately intelligence is usually identified with trainability; trainability is then identified with scholastic progress, and this in turn is identified with verbal knowledge. People who construct and use tests of general intelligence do not always make it clear which they have in mind. Vernon⁽⁵⁾ says that although intelligence appears to be inborn, it 'expresses itself only through the medium of acquired knowledge or "skills"'. He explains the fact that intelligence tests almost always depend on a person's ability to understand oral or written questions by saying that this is really an advantage, 'since one of the most important, if not the most important, uses of intelligence tests is that of predicting scholastic aptitude, and as school work is itself predominantly verbal, verbal tests are certainly the most useful'. Burt⁽¹⁾ goes so far as to say that 'the most abstract relations need words to express them', and that it is only the 'more simple and more concrete which can be conveyed through sensory or perceptual material'. Most people would, however, agree that in the crafts, arts and sciences alike, the more abstract and complex a relation is, the less adequately words will express it. Well-developed verbal ability is perhaps the best indication of scholastic aptitude, but in dealing with the problems of life, thinking in words is often slow, cumbersome and ineffectual. Sometimes it even denotes a maladjustment due to inadequate ability to form clear concepts. Verbal ability is certainly important, as it indicates the information a person has acquired as the result of intellectual activity in the past, and therefore, in many cases, the rate at which he may be expected to acquire information in the future. A verbal test does not for this reason necessarily measure even a person's present capacity for intellectual activity, far less any inborn capacity to acquire skill.

From the foregoing discussion it can be seen that if we have to assess a person's suitability for any form of work or course of training, we should ask ourselves five questions:¹

- (1) Compared with other people, what is his present capacity for observation and clear thinking, and his present rate of mental work?
- (2) What abilities relative to other people has he acquired as the result of past experience?
- (3) What are his chief interests; to what are his activities directed, and how, as a result, does he organize his thought and behaviour?
- (4) Is he determined and persevering; in pursuing his objectives does he show well-defined habits of thought and behaviour, and to what extent does his character appear stable and well-integrated?
- (5) Compared with people of similar intellectual capacities, acquired abilities, interests and perseverance, to what extent is he adaptable?

We are at present able to assess fairly accurately a person's present capacity for observation and clear thinking, and his fund of general information acquired as the result of intellectual activity in the past. In any form of work a person has to undertake, or course of training he wishes to pursue, other factors are important, but these two are essential.

Projective techniques of various kinds enable us to assess with growing accuracy a person's chief interests and concerns, his ideals and wishes for the future, and the resulting ways in which he organizes his thinking and behaviour.

We still need critical and comparative studies of habit formation designed to show the ways in which organizations of conduct become differentiated from one another, attain a degree of functional autonomy, fade, and are later recalled and utilized. Studies in this field will increase our understanding of, and resulting ability to assess, the acquisition and utilization of intellectual and technical abilities, the development of the sentiments and the determination and persistence with which a person pursues his objectives. They will, I believe, also provide information concerning the degree to which the development of habits of thought and behaviour at one period of life or in one generation facilitates or hinders the development of habits of thought and behaviour at a later period in life or in succeeding generations. Whatever the outcome of such studies, they will increase our understanding of both healthy mental development and the formation of abnormal habits and systems of thought. In this way they will be of value alike in Education, Medicine and Social Welfare.

We have at present no way of assessing a person's capacity to adapt to events when he cannot infer their course by analogy with past experience. Adaptability of conduct is clearly a fifth essential quality in all skilful work, while inflexibility of thought and behaviour, even when a person can infer the course of events from past experience, is one of the striking features of mental illness. Unfortunately inquiries into the aptness of skilful conduct have been largely directed to the study of receptor-effector nerve linkages, or to a person's physiological aptitude for specific technical operations. The psychological study of group similarities and individual differences in the ways in which people anticipate events, and respond to them in advance so that they meet them

¹ Alec Rodger (2) in 'Planning for Vocational Guidance' includes, in addition, questions of physical characteristics and circumstances, making seven in all. We are at present only concerned with discussing five basic psychological questions we should ask.

effectively when they occur, has been largely neglected. As a result, the assessment of a person's general adaptability, although extremely important, remains at present almost entirely a matter for future research.

V. INFERENCES FROM TEST DATA

By comparing test performances we can infer a person's past acquisition of information relative to other people, and thence the rate at which he is likely to acquire knowledge and skill in the future. We can also infer variability of conduct in health and illness, the nature and extent of psychological disabilities due to lack of opportunity, maldevelopment or brain damage, and the prospects of future re-adaptation or progressive deterioration. In time we should be able to assess a person's adaptability to given situations and infer how he will tend to participate in events and the integrity of his endeavour. As data accumulate generation by generation we should also be able to infer the degrees to which organizations of conduct, and resulting mental abilities, are passed on from one generation to the next, and can therefore be regarded as inherited or acquired. We should even be able to assess the incidence with which particular abilities may be expected to manifest themselves, and the extent to which they are likely to develop in a given environment. It must, however, be borne in mind that any conclusions we draw are essentially inferences. To speak of measuring 'inborn intelligence' or of testing 'intellectual deterioration' is to confuse test data with the inferences drawn from them. Whatever value words like 'instinctive' and 'intelligent' may have as adjectives, they are misleading when they are turned into names for discrete entities of conduct capable of being enumerated or measured.

Tests can be used to record a person's present output of intellectual work, his present attainments, his interests, the way he organizes his thought and behaviour, and the present persistence and adaptability with which he pursues his objectives.

This is all that seems necessary. The part played by quantitative concepts in the comparative study and assessment of test data still needs to be discussed, but this must be the topic of a separate paper.

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SOME PSYCHOLOGICAL CONDITIONS OF THE DEVELOPMENT OF METHODISM UP TO 1850¹

By W. LAWSON JONES

I. *Introduction* (p. 345). II. *Analysis of building developments in eight selected areas* (pp. 345-347). III. *The psychological influences of local leadership* (pp. 347-349). IV. *Dynamics of the movement, fellowship and fragmentation* (pp. 349-351). V. *The familial structure of Methodism* (pp. 351-353). *Conclusion* (pp. 353-354). *References* (p. 354).

I. INTRODUCTION

The progress of Methodism in its first century of existence has frequently been attributed to the increase and aggregation of the population in England and Wales during the Industrial Revolution. This rough correlation of events has little value save that it raises the question why this widespread shifting of the population should seem to favour a movement like Methodism. This question carries us into the realm of social psychology. In particular it calls for inquiry into the psychological conditions linking Methodism with the social matrix in which it was born.

In order to understand more fully these conditions, it seemed worth while to make a study of the development of Methodism along two lines. An analysis of data relating to the building of chapels in representative areas was the first requirement, and this necessitated a considerable amount of field work. A second line of approach was the study of the structure and ethos of particular Methodist groups, in an endeavour to discover attitudes and institutions which have persisted from early Methodism, upon which they would throw some light.

The historic data were arrived at in three ways: (a) By a search in official records, guides, hand-books, pamphlets, biographies, Wesley's *Journals* and circuit plans. The rich collection of documents in Didsbury and Hartley-Victoria Methodist Colleges and in the Central and Rylands Libraries of Manchester were used. Documents in private possession were also examined. (b) By a considerable amount of field survey. Buildings were sought out and studied, several friends kindly assisting in this work. (c) By a study of the chief histories of Methodism.

The second method required a more personal approach to certain selected groups in different parts of the country and involved a close study of them over a period of several years.

II. ANALYSIS OF BUILDING DEVELOPMENTS IN EIGHT SELECTED AREAS

Since a detailed study of the whole of Methodism was out of the question, we selected eight areas for detailed examination. The grounds on which these areas qualified for inclusion in our survey were as follows:

(a) We already had a fairly extensive acquaintance with Methodism in each of these areas and therefore had some data to begin with.

(b) They were representative of the wider whole of Methodism both geographically and in the sense that city, town and rural areas were included.

¹ Based on an investigation carried out at Liverpool University.

(c) They straddled across the main line of the spread of early Methodism from Bristol to Newcastle, and give a general picture of that development.

Two matters need explanation. First, we preferred to study the building of chapel rather than the increase of membership because the former gave a more sure indication of the strength of the movement. Membership is unstable. Buildings cannot be maintained without solid support. Secondly, the areas mentioned in Tables 1-5 were strictly coincident neither with geographical nor with Methodist boundaries. The important thing is that in each of the tables the same area is covered by the same name, making comparison possible. The first table indicates the state of things when Methodism arose, and, when compared with the rest, suggests some reasons why it developed more slowly in some places than in others.

Table 1. *Churches established before the eighteenth century*

Area	Anglican	Dissenting	Methodist
Bristol	18	6	None
Newcastle	4	3	None
Birmingham	3	5	None
Berkshire and Wiltshire	29	10	None
Liverpool	6	4	None
Manchester	7	17	None
Denbighshire	17	3	None
West Riding	15	6	None

Table 2. *Churches established during the eighteenth century*

Area	Anglican	Dissenting	Methodist, including Calvinistic Methodists	Increase in the population, 1700-1790
Bristol	5	8	10	71,000
Newcastle	3	4	3	25,000
Birmingham	3	7	5	26,000
Berkshire and Wiltshire	7	18	11	105,000
Liverpool	4	7	3	36,000
Manchester and Salford	7	21	8	36,000
Denbighshire	6	11	7	30,000
West Riding	11	12	25	200,000

Table 3. *Churches established during the first half of the nineteenth century*

Area	Anglican	Dissenting	Methodist including Calvinistic Methodists	Increase in the population, 1800-1850
Bristol	1	5	25	150,000
Newcastle	3	9	14	60,000
Birmingham	5	13	20	225,000
Berkshire and Wiltshire	9	15	40	170,000
Liverpool	5	16	19	330,000
Manchester	8	10	14	375,000
Denbighshire	10	23	42	32,000
West Riding	10	89	153	1,200,000

N.B. A few Roman Catholic and Jewish Churches were established in some of our areas during the period under review; but their incidence did not affect the development of Methodism.

To bring out the fact that increase in population was not itself the direct occasion of the increase in Methodism, the areas are put in the ascending order shown in Tables 4 and 5 according to the number of the new population to each church built.

An inspection of these tables brings out the surprising fact that Methodism did not always advance most rapidly where the increase in the population was greatest.

In Table 2, Berkshire and Wiltshire and the West Riding should have shown a greater increase of churches had mere aggregation of population determined development. Table 3 suggests that, in Birmingham, Liverpool and Manchester, Methodism grew very slowly in the first half of the nineteenth century, in spite of the tremendous development of these great industrial centres. Factors other than the growth of the population were at work. The generally accepted view that Methodism thrived on the unfranchised masses needs qualification. Curiously enough, this statement appears to be less true of the early nineteenth century when the growth of the population was accelerating, than of the

Table 4. *Number of additional inhabitants (in round figures) to each Methodist Church (including Calvinistic Methodist) built during the eighteenth century.*

	One church to every	2,700 new inhabitants
Denbighshire		4,500
Manchester	"	5,200
Birmingham	"	7,100
Bristol	"	8,300
Newcastle	"	9,500
Berkshire and Wiltshire	"	12,000
Liverpool	"	16,600
West Riding	"	

Table 5. *Number of additional inhabitants (in round figures) to each Methodist Church (including Calvinistic Methodist) built in the first half of the nineteenth century.*

	One church to every	750 new inhabitants
Denbighshire		4,500
Berkshire and Wiltshire	"	5,400
Newcastle	"	6,000
Bristol	"	8,000
West Riding	"	11,250
Birmingham	"	17,315
Liverpool	"	26,800
Manchester	"	

eighteenth century when the increase was not as prodigious. John Wesley's⁽²²⁾ *Journals* contain not a few hints that, after the first flush of revival, progress slowed down. The records of the Kilhamite and Warrenite controversies suggest, and the official figures for the period 1790–1800 show, that progress did not accelerate again until well into the nineteenth century.

The only conclusion one can draw from Table 4 is that Methodism did not make much headway before 1800 in such agricultural and partly urban areas as Wiltshire, Berkshire and the eighteenth-century West Riding. Table 5, on the other hand, clearly suggests that the most striking progress between 1800 and 1850 was made in these rural and urban areas such as Denbighshire, Wiltshire and Berkshire, and the West Riding, whilst the growing cities, Birmingham, Liverpool and Manchester, lagged behind. This is in keeping with such official figures as are given by Dr Maldwyn Edwards⁽⁶⁾ in his *After Wesley*, where from 1815 onwards the greatest increases in membership are shown to be in the smaller towns and villages.

We must now look for an adequate reason for these unexpected phenomena.

III. THE PSYCHOLOGICAL INFLUENCES OF LOCAL LEADERSHIP

The first key to the understanding of the situation outlined is the influence of the local lay leader. He has always been of very great importance in Methodism. Wesley's organizing genius notwithstanding, it is doubtful whether Methodism would have survived the shocks it received towards the end of the eighteenth century but for the loyalty of

local leaders. Where well-established Dissenting churches commanded the adherence of the managerial and trading classes, from which Nonconformist leadership was so largely drawn, Methodism had to wait until it could attract people stable and intelligent enough to serve as local leaders. Methodism made slower progress, also, in places from which local lay leaders were drawn away by the business opportunities in newer communities.

The Established Church made hardly any attempt, in our period, to meet the changing conditions of the Industrial Revolution. No bishop had been appointed in the West Riding until 1836 or in Manchester until 1847, and in 1806 the number of non-resident incumbents was stated in Parliament to be 2423 (11, 13, 9, 6, 8). Our tables indicate how few parish churches were built in comparison with the Nonconformist chapels. Neither Anglicanism nor Sidmouth's political and judicial attempts to restrict lay preaching interfered with the growth of Methodism (4). But Dissent had a firm grip upon many of the growing towns. Where it waned, as in parts of Berkshire during the later part of the eighteenth century, Methodism stepped into its place. But where it held the field Methodism, energetic as it was, did not keep pace with the growth of the population. This was the situation in the early part of the nineteenth century, in Manchester, Birmingham, Liverpool and even the birthplace of the Methodist Revival, Bristol; all acknowledged Dissenting strongholds. In Denbighshire, Calvinistic and Wesleyan Methodist chapels were built in extraordinary numbers, in spite of the concurrent growth of Dissent. The number of large chapels, often in small villages, in this county strikes the eye of the visitor even now that many have disappeared. An investigation of the origin of the smelting, mining and quarrying industries in this area and a study of such authorities as Prof. A. N. Dodd's (5) *Industrial Revolution in North Wales*, Palmer's (15) well-known *Older Nonconformity of Wrexham and Neighbourhood* and such details as are to be found in a number of local church records¹ suggested that this growth of Methodist buildings was due to the uprise of new mining, smelting and quarrying villages where Dissent had had little opportunity of getting in before Methodism. Judging by the number of Methodist societies founded about 1810 and again about 1820, something like a local revival must have occurred in or near these years. Palmer (15), the Wrexham historian, hints at this and there are records of the mission of Thomas Russell, the itinerant preacher, in 1820. The histories of many village societies, usually published in Centenary Handbooks, preserve the names of large numbers of lay leaders brought into Methodism by such Missions in this area. At the same time Manchester was being drained of this type of lay worker who was going out into the surrounding towns to found new causes there (14, 7). Relatively small progress, then, was made in Manchester in the early years of the nineteenth century.

Understanding of the progress of Methodism can best be sought, then, by first asking: What was the psychological significance of the local lay leader?

He or she was a person capable of arousing, intensifying and directing the affective responses of the members. When societies were organized into circuits with a resident minister living at the centre and responsible for the general oversight of a number of societies in the district, it was impossible for the minister to do more than a small fraction of the preaching and pastoral work and he was fortunate if he succeeded in keeping some control over administration. Lay leaders were freer in some senses than i

¹ The late Rev. E. K. Jones, D.D., an acknowledged authority on the history of Denbighshire, placed his records at our disposal.

Wesley's earlier days when the connexional leaders itinerated and maintained a stricter general oversight. The door to lay leadership was opened widely. Ethical requirements there were. But, given these, emotional volatility seemed a chief desideratum. Women had almost equal opportunity with men, especially in the class-meetings. Eccentricity was sometimes taken to be a mark of inspiration. Here and there the mother of a large family or the grandfather of a number of families assumed leadership. At times of revival, prayer leaders were appointed in some parts of the country to whip up enthusiasm. The leader, then, became the pivot of a field of psychical energy which, when it was worked up into explosive intensity, produced an impact upon individuals and the community frequently resulting in complete changes of individual or social habits.¹ This situation, it is easy to see, was highly favourable to revivals. But it does not at first sight account for the way in which Methodism became so firmly established amongst the neglected populations. For a convincing answer we must examine the disposition of the society centred around the leader or, more usually, the leader and his family.

IV. DYNAMICS OF THE MOVEMENT, FELLOWSHIP AND FRAGMENTATION

There was a marked contrast between the psychological atmosphere of the outside world and that within the Methodist society. In industry and commerce a harsh competitiveness dominated everything. A characteristic of Methodism was its fellowship: close and continuous by reason of the direct daily contact with the members which a local leader could enjoy. The familial structure of the societies produced a close rapport between the members. Such fellowship offered recognition, companionship, affection, to the most unimportant person. The situation of the child in the family was reproduced. To be 'in society' was to belong to the Kingdom of God or 'to be saved'. This sense of love and security is reflected in the doctrines most characteristic of Methodism, the doctrine of assurance (of salvation) and the doctrine of perfection which proclaims the possibility of a life of perfect love on earth. Methodists were usually affectionate in their relations one with another. John Wesley himself was noted for the way in which he expressed his fondness, especially for children⁽²⁰⁾.

It is plain that the keynote of the Methodist appeal was tenderness; a tenderness which could only be realized within the close rapport of a family society. That is why Methodism flourished only where its work centred around the life of a family or group of families—a matter which we shall stress later. Since millions of people responded to this appeal as to nothing else in life, it may be taken that tender affection, a social urge, is *one of the* most urgent needs of human nature.

Further features of maturing Methodism manifest the close rapport within the societies. I refer especially to its radicalism and its fragmentation. Conservative tendencies in Methodism need no explanation. They derive from Wesley. It is in the radical sympathies of Methodism we hear the people speaking. For the politico-religious radicalism of the nineteenth century owed far more to class conflict than to theory. Much of it was due to the opposition between the egalitarian character of Methodism and the quasi-feudal tendencies of the squire and parson. From experience one can testify to the intense hostility which could spring up between the people gathered round the Methodist chapel

¹ A comparison between the effects upon individuals of religious revivals and those of the electric-shock treatment in psychiatry would be interesting. See *Physical Methods of Treatment in Psychiatry*: Sargant & Slater⁽¹⁶⁾, and W. L. Jones⁽¹²⁾: *A Psychological Study of Religious Conversion*.

and those of different organizations. The closer the rapport in the Methodist society the greater the tension with opposing groups. This hostility was but the obverse of loyalty to one's 'cause'. The religious loyalty gathered to itself political sympathies: and so the Methodist society often became markedly radical in politics, in reaction against the conservatism of the Established Church. Denominational leaders occasionally had to restrain this radical tendency. For the inarticulate interests of the masses, seeking vent where they could find it, could easily divert the Methodist pulpit from its proper religious use. Paradoxically the close rapport between members in the societies also favoured conflict when occasion arose. Members were not insulated from one another by strangeness or indifference. They suffered less restraint in expressing feelings and took greater mutual liberties than would people who were less familiar. When irritation or annoyance arose, it was more apt to explode into open quarrels and less restraint was shown in controversy, since the delicate inhibitions of a more polite and formal association were absent. Hence, not long after Wesley's strict rule was terminated by death, fragmentation occurred in many parts of Methodism. Often families were divided against themselves. Disgruntled individuals found defection to the 'splits' a temptation. Thus a number of derivative Methodist bodies grew up. The over-building which occurred in Methodism in the middle of the nineteenth century was in part due to this fragmentation. Neither in Anglicanism nor Dissent did it occur to anything like the extent it did in Methodism, because the contact of members in these churches was not so intimate, and hence the opportunity for friction was less. Another symptom of over-intense rapport within the local societies was the anti-ministerialism which occasionally broke out, in the same way as the anti-clericalism of some of the Franciscan communities in the thirteenth century. Catholic ritual, as Pope Pius X reiterated, aimed at becoming an effective safeguard against the dangers of too great an intimacy. Prof. Romano Guardini⁽¹⁰⁾ has explained how the liturgy provides 'a regulation of religious life in common, with, at the same time, a view to actual needs and requirements'. Though not entirely lacking a liturgy, Methodism regarded ritual with suspicion and used it less and less, even though the founders had been sacramentarians. Rather did it seek to solve the problem of dangerous tensions in a practical way. It turned the energy and enthusiasm of its people into the channels of evangelism, missionary enterprise, humanitarianism and philanthropy. Many local histories like J. Smith's⁽¹⁸⁾ *History of Liverpool* (1810) bear witness to such benevolence. The persistent calls upon the generosity of its people might have killed local enthusiasm. On the contrary, however, an increasing volume of voluntary contributions poured into the many religious, educational and philanthropic enterprises sponsored by the movement. The full spate of this bounty did not come until the twentieth century, when British Methodists alone were providing over £5,000,000 annually for all purposes. Yet even as early as 1822, writers like J. Batty of Rothwell and W. Wilson of London mentioned enviously the incomes of Methodist societies⁽⁶⁾. It is clear that the very call for personal sacrifice tended to obviate any jarring feelings and to bind members to those who called forth such a spirit of charity. Thus Methodism succeeded fairly well in holding its people together in spite of the stresses inherent in an intimate society. Only the most compulsive motive could have inspired this sustained generosity, often most willing where the means were least. Construed in terms of tender affection, this becomes intelligible. It is difficult to account for it by a Freudian etiology.

The evidence that Methodism had profoundly socializing influences upon individuals

organizations and communities, has been competently put together by Drs Robert Wearmouth⁽²¹⁾, Maldwyn Edwards⁽⁶⁾ and E. D. Bebb⁽²⁾. When one remembers that the Methodist movement was concerned with the lives of millions of ordinary English people, one is led to wonder whether the deepest and most powerful motives in man are not social rather than erotic. At any rate it must be said that only tender affection could have sustained the service and generosity we have noticed, a tender affection which is at the root of human nature. Are the deepest elements in human nature inherently social? Methodism seems to have drawn upon a human energy which was both fundamental and social, a tender affection which, given adequate expression, was able first to create and then to hold together whole communities. There can be no doubt, at any rate, that the magnet which drew socially neglected people together and so built up Methodist societies in the period we have reviewed was the emotional satisfaction of fellowship. In saying this, we must not be taken to suggest that other factors were entirely unimportant. This was not so. Aesthetic, recreational, ethical and intellectual interests played a major part in the appeal Methodism made to the unprivileged. Lacking the amenities of the cities, the populations in the new areas made the Methodist chapel their social and cultural centre. Thousands of people learnt to read, write, speak and carry on discussions in the Methodist Sunday Schools^(22, 19). Often the chapel offered the only recreational alternative to the public house and cock-pit. 'Sermon tasting' and hymn-singing became popular pleasures comparable to the present-day cinema. Further, intellectual hunger and the need for moral guidance were widely felt. Unadorned by great culture, Methodist worship offered preaching and teaching shaped by the clear logic and sound ethics of John Wesley's sermons and Charles Wesley's hymns, and this stirred the mind as it moved the heart. Converts, intoxicated with the new wine of truth, felt no reserve in offering to outsiders their happiness. It is true that in offering so much to those who possessed so little, Methodism won territory which other churches would not or could not take.

But the force which enabled Methodism to live and grow was its fellowship. None of its cultural activities would have established the movement as an abiding element in British life without the warmth of its fellowship, which appealed to the deepest affections and needs of men.

V. THE FAMILIAL STRUCTURE OF METHODISM

Turning now to the structural aspects of Methodist societies, we must amplify a statement made earlier to the effect that not only the leader but his family held an influential place in the society. So many Methodist societies, especially in smaller places, depended upon one leading family.

Now, the family is much more than an aggregate of individuals. It is a unit. Its affective attitudes are more persistent, especially when they relate to religious, ethical or political matters, and its power of propagating such attitudes is greater than that of any mere collection of unrelated individuals. An individual's mood may change when the general atmosphere of the home remains the same. Thus the family often kept its members loyal to Methodism when, left to their own feelings, they might have lapsed. Further, the family is prone to hospitality and sociableness, the lonely individual to a more self-centred life. Methodist hospitality has evoked both the envy and amazement of other denominations for more than a century. In our period, it was the common practice for

a group of families to meet in one of the homes after service or on a week-night. Methodism freely employed all the advantages of family life and owed more than can be tabulated to this fact. All over the country, there are villages where thriving societies waned and finally ceased to exist after the leading family had moved away. Greenham and Donnington in Berkshire, Minera and Newtown Mountain in Denbighshire, the day-school at Worsley Road, Walkden in Lancashire (where for several generations the same family supplied the teaching staff) are examples of hundreds of like instances in the records of the Methodist Chapel Affairs Department. One big reason why the increase of chapels lagged behind the growth of the population in some of the developing industrial areas is that it took some time for family life to get established. Small business-men who had risked their capital in moving to a new place where goodwill needed to be built up from the beginning, required to give individual attention to business for some years so as to become well established. Their families, also, could not jump into a community and take a leading part therein without first getting to know their neighbours well. Thus it took time for Methodist families moving into strange surroundings to establish themselves sufficiently to organize religious activities. This in part explains why the growth of the population out-paced the growth of Methodist societies in certain areas. The family was the nerve-centre of the religious society; and where the latter was to be stable the former must be well-established. Methodism, in the nature of things then, became familial in structure and government.

Three advantages of such an organization over movements which reject the family (e.g. monastic and communist bodies) stand out. Community life, with the love, recognition and sense of incorporation it conferred upon the individual—something akin to the sense of atonement—could be enjoyed without disrupting everyday life. Some of the values of monastic life were gained, therefore, without forsaking the everyday world to gain them. Again, the Methodist society could and usually did control its members by a familial compromise so that force was rarely necessary (in contrast to Communism where enforced discipline seems indispensable to the system). John Wesley laid down rules for his societies which were revised by successive Conferences. These the superintendent ministers of the circuits were required to enact in the societies. But the almost invariable practice was to instruct the lay leaders and leave it to them to see that the rule was obeyed. Difficulties and, on rare occasions, open resistance occurred, as the controversies which can be traced in the collections of Methodist pamphlets and documents in the possession of the various Methodist Colleges show. It is remarkable, however, that there was not more conflict. The vast majority of Methodist societies were loyal to the Connexion, and there can be no doubt that this loyalty was secured by the local lay leader and his family. Often the leader as the father or grandfather of a large family or group of families could secure adherence by persuasion, where ministerial authority would have failed. The lay leader not infrequently protected the minister from anti-ministerial feeling by acting as the 'go-between', linking the local society with the circuit minister and wider Methodism and handling any business likely to cause hostility to the connexional authority. When Jabez Bunting, Robert Newton and Thomas Jackson, Methodist ministers, were threatened by the 'Yorkshire Luddites' whom they successfully restrained from extensive bloodshed, they were accompanied to their appointments in the villages and defended in the societies by lay leaders. Both the *Journals* of John Wesley and the biographies of Hugh Bourne, J. J. Gurney and other

pioneers of Methodism are full of references to the lay people who kept up the enthusiasm and loyalty of the society members. Since these members largely consisted of craftsmen, artisans and the more intelligent workers, who had influence over the less articulate members of the community, it is clear that the influence of the Methodist lay leader extended to the social and political sphere⁽⁵⁾. Dr Robert Wearmouth⁽²¹⁾ has amassed the evidence for this powerful influence in his *Methodism and the Working-Class Movements of England*. In his recent study of the Methodist people, Dr Leslie Church⁽³⁾ has provided most carefully checked and intimate details which supplement Dr Wearmouth's evidence. It was the contention of Dr J. S. Simon that Methodism saved England from the fate of revolutionary France and created a new nation⁽¹⁷⁾. Historians like W. E. Lecky and J. R. Green go some way in support of such a claim. It can scarcely be doubted that Methodism exerted a steadying influence over what might well have become a revolutionary element in the population. Methodism developed, in our period, a compromise between the radicalism of the rank and file and the conservatism of the leaders which has continued to this day. This compromise was made possible by the exercise of authority parentally through family influence rather than by official command.

A further advantage family life contributed to Methodism was its propagative value. Compact and stable, when it emigrates, the family usually increases its coherence and the intensity of its characteristic attitudes. The well-known patriotism of emigrant communities in the Commonwealth exemplifies this tendency. The emigrant family, or the family establishing itself in a strange district, brings to the new environment its own 'background' and tends to propagate its ideas and manners. It thus provides a centre through which a movement commanding its adherence can propagate itself. Emigrant groups from Wales gave an impetus to the Methodist Revival in Bristol and along the Welsh border. Similarly, to-day, according to the reports of such authorities on missionary work in India as the Rev. Cyril Eastwood⁽¹⁾ of the South India Church, family life is a powerful agent of both Christian and Hindu culture, whereas Communism, neglecting the family, is winning comparatively few adherents.

As we have hinted, the constitutional and doctrinal development of Methodism were both dominated by the psychological conditions outlined above. The familial character and intimacy of Methodist fellowship has bred a constitution in which a balance is held between authority and democracy. Doctrine, stressing the new birth, assurance of salvation, sanctification and the life of perfect love, clearly reflects the spirit of small, intimate societies where people are immediately confronted both with aids to the Christian life and with problems they could personally solve. By contrast, in modern society, where neither the problems of life nor the spiritual resources are so accessible—because of the bigness of society—religious doctrine is apt to become less concrete and clear.

VI. CONCLUSION

Mere aggregation of population was not in itself responsible for the uprise and growth of Methodism in the eighteenth and nineteenth centuries. Making its appeal to one of the deepest needs in human nature, tenderness, Methodism seems to have grown most rapidly where the means of ministering to this need were at hand. This ministry centred round the local lay leader's family and the familial life of the local society. This meant that Methodism worked with families and not merely with individuals, and so secured greater

stability and scope. The Methodist revival probably owed its spread to some extent to the emigration of evangelical families from old homes to the new industrial areas.

Familialism produced internal intimacy, tension with outside groups, philanthropy and, when friction did occur within the societies, fragmentation. On the whole, it led to a compromise between conservative authority and a radical membership. This situation not only encouraged rapid growth in areas where the requisite leadership was present, it dominated the development of Methodism as that is reflected in its constitution and doctrine.

Aesthetic and intellectual values of Methodist worship, added to the psychological and social values of its fellowship, were such as to meet a most widespread need in nineteenth-century England.

In its social structure, psychological atmosphere and intellectual expression, Methodism matched the needs of the new communities of semi-industrial and industrial England and Wales for whom the older churches were unable to provide in any adequate way.

Our study suggests the general conclusion that tender affection with its conative aspect in companionship is one of the prime motives of social life. It is greatly reinforced by fundamental urges which find ready expression in aesthetic, intellectual and ethical satisfaction.

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THE MEASUREMENT OF TIME SEQUENCES IN CONVERSATIONAL BEHAVIOUR

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I. *The problem* (p. 355). II. *The method* (pp. 355–356). III. *The hypothesis* (p. 356). IV. *The experiment* (pp. 356–357). V. *Treatment of data* (pp. 357–358). VI. *Results* (pp. 358–359). VII. *Further analysis of data* (pp. 359–360). VIII. *Results of second analysis* (pp. 360–361). IX. *Summary* (pp. 361–362). *References* (p. 362).

I. THE PROBLEM

Although conversation is one of the most important forms of social intercourse and is the basis of such a widely used technique as interviewing, little work has so far been done to throw light upon the fundamental nature of the process of two people conversing. This paper describes the first of a series of experiments which have been conducted to study some aspects of conversational behaviour.

There are two kinds of data with which we have to deal in conversations: those which may be summarized as (a) *what* the speaker says, and (b) *how* he says it. The first is the content, data of a highly qualitative kind which, in spite of many brave attempts at classification, tend to prove unwieldy and unruly in the process of being forced into categories. The second is concerned with the form in which this content is presented: the expressive movements accompanying it, gestures, facial expressions, and mannerisms, the speed of talking, the pauses, silences, interruptions, the rate of talking and so on.

Experienced interviewers, for instance, pay great attention to the clues provided by these formal aspects, i.e. to the 'how'—as distinct from the 'what'—aspects of the things said. A number of these aspects are functions of time, which is an objectively measurable quantity. Therefore whoever wants to explore this confused field with the purpose of studying it scientifically, might in the first place profitably concentrate on just this aspect of social behaviour. How long at a time a person speaks without a break or remains silent during the course of the interview, how frequently he starts to speak and falls back into silence, how often he interrupts his partner and for how long at a stretch, how long and how frequently the persons engaged in a conversation or interview are mutually silent or else both talking at once—these can all be measured in units of time.

These data and their various relations were claimed by Chapple^(1,2) to be significant for the diagnosis of certain personality traits as well as of the various mental disorders. The present research was prompted by the promise of this approach.

II. THE METHOD

Chapple's method of interview analysis consists fundamentally in recording on a chronograph tape the durations of actions (speech and gesture) and silences of both persons involved. Apart from talking, those kinds of gesture were interpreted as actions which were of a response character or part of the social interaction process, such as nods, certain kinds of smile, or shaking of the head, and laughter. Although this is a matter of judgement, preliminary recording by two observers in the present investigation showed no essential differences in the interpretation, once the criteria were agreed upon.

Two press buttons, each setting off a separate pen drawing lines on a tape, are allotted to the two people in the conversation. As soon as a person speaks, nods, or smiles in response, the corresponding button is pressed down by the observer; as soon as this person falls into inaction it is released (2). The lines drawn on the paper thus record the durations of the successive actions and silences for both partners in the conversation. These measurements are the basis for derived measures, such as the total amount of time of action or silence, the frequency or number of actions or silences, the average duration of actions or silences, the amount of time spent in mutual silence or double action, and the various relations arising from these data. Each action with its subsequent period of silence together form an interaction unit, so-called because the silence coincides normally with the response of the partner.

III. THE HYPOTHESIS

In applying the method described above to the study of conversations and interviews, Chapple made the inference that 'each individual had constant and invariant relationships in the frequency of his actions and inactions no matter with whom he interacted'.

The implications of this claim must be judged to be far-reaching: for to demonstrate, and isolate from the reality of social interaction, such a constant property as a function of objectively measurable quantities, is an important step forward in the development of the scientific analysis of the interviewing process.

Our present experiment represents an attempt at verifying, in ordinary conversation, that when two people are talking together, there is a relatively permanent tendency for each either to maintain long silences, or to talk at some length for continuous periods, or to break silences frequently.

IV. THE EXPERIMENT

Seventy-one conversations, each of 30 min. duration, were recorded on a chronograph tape. They consisted of seven series of about ten conversations (varying from nine to twelve), between seven permanent speakers and different partners. The subjects were members of the Psychology Department of Maudsley Hospital. Each record shows the length of the various periods of activity (talk and gestures) and of silence of the two partners.

The choice of subject was left free; and the only instructions regarding the content were (a) that it should be of common interest to both partners, (b) that it should be of such a nature that both were reasonably conversant with it, and (c) that both partners should possess fairly strong views concerning it.

As was to be expected, not all of the conversations recorded came up to this requirement equally well. Four conversations were disqualified before analysis because the wide difference in the amount of information possessed by the two partners on the topics discussed made them into lectures.

A weakness of this experimental design was the fact that the groups of partners in conversation were not the same for all constant speakers. While some partners took part in several of these discussions, on the whole each partner group had a different composition. This could not be helped but it means that the stimulus background varied from subject to subject.

It is to be expected that an ideal experimental situation would yield results of greater

rather than less significance; an expectation which we found fulfilled in a second more rigorously controlled experiment (4).

V. TREATMENT OF DATA

The measurement of the periods of action and silence in terms of tenths of seconds provided the raw data. Frequency distributions for both actions and silences (with the frequencies on the ordinate and the durations on the abscissa) were, as had already been observed by Chapple, J-shaped.

An inspection of the material suggested that where the actions and silences were rather few and far between, that is in the durations of more than 20 sec., individual differences might reveal themselves most distinctly. For the differential capacity of people to speak or to keep silent for long periods seemed to have psychological meaning in terms of everyday experience.

It was decided to classify the material into four groups. Short actions which we call *ba*; short silences, *bs*; long actions, *da*; and long silences, *ds*. 'Short' means periods up to 20 sec., and 'long' periods of 20 sec. and more. The designations *ba*, *bs*, *da*, *ds*, are taken from Chapple, and are presumably derived from the Latin *brevis* and *durare*.

The following considerations determined the choice of our measurements. Conversational activity divides into two aspects, action (i.e. speech and gesture) and silence. The relative frequencies of long actions in comparison with long silences, i.e. the proportion of the periods during which a person talks at great length to those during which he relapses into silence, might be expected to give a measure of his urge for activity. Mathematically this might take the form of a ratio da/ds , *da* being the number of actions longer than 20 sec., and *ds* the number of long silences. Obviously this is a crude measure; for a wide range of actions or silences are classed under *da*, or *ds*, from 20 to 100 sec. and sometimes more.

If, as the hypothesis postulates, individuals are consistent in their conversational activity over and above the fluctuations from situation to situation and from partner to partner, and if further the da/ds ratio is a reliable measure or score of one such consistent aspect of activity in conversation, the expectation is that the da/ds scores for any one individual should vary over a smaller range than they do when derived from the records of different individuals. If we assume that this score is determined not only by some constant aspect of the individual's conversational personality but also by the stimulus situation (different partners, topics, changes of mood), the following may be deduced: that the scores for the seven groups of partners, who in each of the seven series converse with always the same constant speaker, are more stabilized with respect to the stimulus situation than the scores for each of the constant speakers who talk to different partners. If we expect that under these circumstances the distribution of scores for the constant speaker is more compact than that for the different partners, we imply that in comparison with the forces working *within* the speaker the stimulus situation (topic and partner) is of negligible import in determining the measure of his conversational activity.

In order to examine whether the above expectation is borne out, it was necessary to compare the relative variabilities of the two groups. A frequency distribution of da/ds ratios for the total sample of partners as well as constant speakers was clearly J-shaped, and it was therefore not possible to employ the usual tests for comparing differences in variation as these rest on the assumption of normality in distribution.

Therefore the determination of the shape of the distributions itself was chosen through the method of moments, to illustrate any differences of range and spread between constant speakers and partner groups. It is fully realized that this treatment allows for no inference with respect to the population at large, but that it merely provides a descriptive account of the actual observations made in every case (4).

The shape of a distribution is defined by the third and fourth moments, or β_1 and β_2 the measures of skewness and kurtosis.¹ For the normal distribution $\beta_1 = 0$, and $\beta_2 = 3$.

VI. RESULTS

Table 1 shows the β_1 and β_2 constants of da/ds for the seven constant speakers in the experiment and their partner groups. In every case the partner distribution shows greater skewness than that of the constant speaker, and in every case but one considerably greater kurtosis. The samples were too small to permit a test of normality to be made, but using Pearson's Diagram (5) to determine from the knowledge of β_1 and β_2

Table 1. β_1 and β_2 values for the distributions of the da/ds scores

		Speakers								
		S	I	G	M	Y	F	W	Mean	S.E.
β_1	I	0.797	0.877	0.072	0.022	0.479	0.891	0.283	0.489	0.141
	Gr	4.238	3.939	4.604	4.429	2.398	2.439	1.100	3.306	0.504
β_2	I	2.840	2.570	1.590	1.070	1.590	2.130	1.730	1.931	0.234
	Gr	5.990	5.400	6.304	6.290	4.160	4.250	1.570	4.852	0.643

I = 'Individual' score; Gr = 'Group' score.

the appropriate types of frequency distribution it was immediately obvious that the distribution of scores derived from the same individual (or, as we shall call them, the 'individual' scores), from whichever constant speaker they originated, were best described by one type of universe, while the distributions of the 'group' scores coming from the various groups of different people (the partners) were best fitted by a characteristically different type. The Pearsonian frequency curve Type II was the nearest approximation for the 'individual' scores in six out of the seven series, and Type VIII, again in six out of the seven series, was the best fit for the 'group'.² The outstanding fact relevant to the hypothesis is that out of the seven sets of conversations in six sets we find the 'individual' scores distributed symmetrically and within a limited range, while the 'group' scores are spread out in a highly skewed manner with the majority of scores clustering at one end of the distribution. This latter form corresponds clearly to the distribution of da/ds scores in the total sample as illustrated in Fig. 1. The typical score therefore is given by the mode rather than the mean, and it seems that the majority of people have scores below 2.5 in conversational activity with a small proportion of exceptional individuals ranging widely in the higher values. The symmetry of the distribution of 'individual' scores, on the other hand, seems to indicate that *within* an individual a central tendency asserts itself. A few of the 'individual' score distributions

$$^1 \beta_1 = \frac{(\sum X^3)^2}{N^2 \sigma^6}; \beta_2 = \frac{\sum X^4}{N \sigma^4}.$$

² Type II of the Pearsonian frequency curves is, according to Elderton's table and classification (3), a symmetrical distribution of limited range (5)—very similar in shape to the Gaussian normal curve, but flatter and more compact. Type VIII is characterized by skewness and peakedness.

showed a U-shaped bend possibly induced by a differential reaction to male and female partners of which there were equal numbers in each sample.

While no conclusions can be drawn from the above as to the significance of the differences in the distributions, the fact that the differences between the β_1 and β_2 constants of individuals and groups are consistent practically all through the experiment, is highly suggestive.

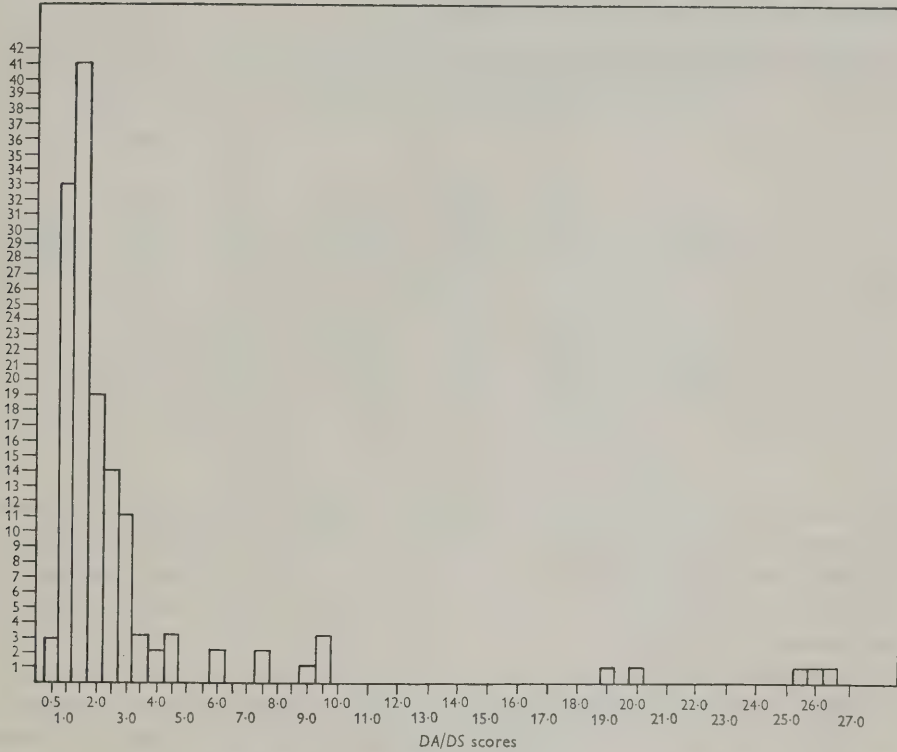


Fig. 1. Distribution of *da/ds* scores from seventy-one conversations.

VII. FURTHER ANALYSIS OF DATA

The *da/ds* ratio which we examined above masks two components relating to two different aspects, or dimensions, of conversational behaviour; activity and silence. The question is whether the consistency of individual behaviour in conversation, which we found suggested by the symmetry and compactness of the distribution for individuals, is an aspect of their interrelatedness or whether the action pattern or the silence pattern separately constitutes the stable and specific element in conversation.

Activity seems to be more easily interfered with by an active partner, or adjusted as a matter of conscious control, than the silence pattern of an individual, i.e. the length and frequency of his pauses. Pauses or silences, on the other hand, cannot be expected to be so easily modified. A tendency for keeping long silences can be prodded into greater activity only with much greater effort—even by a deliberately stimulating approach—than a tendency to be active can be inhibited. On the other hand, a person who is not capable of keeping inactive for long may be prevented from talking at length, or even from entering the field of conversation to any great extent, but his incapacity for long

listening will find expression in short spurts of activity such as interruptions, short interceptions, or gestures. It was therefore felt that of the two components of our ratio da/ds the silences might be the operative term rather than the actions.

To test this assumption two further ratios were computed: the ba/da ratio representing the relative frequencies of short and long actions, and the bs/ds ratio which takes account only of short and long silences. The $\beta 1$ and $\beta 2$ constants for the distributions of these values were calculated and are given in Table 2.

Table 2. $\beta 1$ and $\beta 2$ values for the distributions of the bs/ds and ba/da scores

		Speakers							Mean	S.E.
		S	I	G	M	Y	F	W		
		<i>bs/ds</i>								
$\beta 1$	I	0.69	0.27	0.10	0.16	0.34	0.46	0.24	0.323	0.076
	Gr	5.08	3.71	3.95	6.29	2.64	1.85	0.45	3.281	0.845
$\beta 2$	I	2.17	2.15	2.56	1.73	1.56	1.60	1.73	1.930	0.137
	Gr	6.48	5.20	5.72	7.66	4.31	3.57	1.93	4.960	0.743
		<i>ba/da</i>								
$\beta 1$	I	0.73	6.76	0.70	0.49	1.03	0.28	1.63	—	—
	Gr	1.85	0.07	0.03	2.62	0.16	0.03	2.35	—	—
$\beta 2$	I	2.98	7.92	3.06	2.51	3.71	3.87	3.77	—	—
	Gr	3.33	1.80	2.05	4.42	2.87	1.62	4.16	—	—

I = 'Individual' score; Gr = 'Group' score.

VIII. RESULTS OF SECOND ANALYSIS

From Table 2 we can see that the bs/ds score discriminates clearly between the distributions of scores coming from the same speaker (individual scores) and those coming from different people (group scores). The ba/da ratio, on the other hand (Table 2), shows no consistent tendency whatever for the third and fourth moments to fall into two different groups of any sort.

We may therefore conclude that the stable element in the time pattern of conversations of individuals is to be found, not so much in those measures which are concerned with their active behaviour, as in those which take account of the intervals of inactivity between the periods of action. As far as the measurements reported here are concerned, it was found that the two measures containing in their denominator the number of ds periods, i.e. periods of silences lasting over 20 sec., proved sensitive to variations, while those containing long actions (da), were poor discriminators.

Table 3. *Significance of the differences between the mean values of $\beta 1$ and $\beta 2$ for the 'individual' and 'group' score distributions of the da/ds and bs/ds ratios*

		<i>da/ds</i>				<i>bs/ds</i>			
		Mean	S.E.	<i>t</i>	<i>P</i>	Mean	S.E.	<i>t</i>	<i>P</i>
$\beta 1$	I	0.489	0.141	5.4	0.01	0.323	0.076	26.1	0.001
	Gr	3.306	0.504			3.281	0.845		
$\beta 2$	I	1.931	0.234	4.3	0.01	1.930	0.137	4.01	0.01
	Gr	4.852	0.643			4.960	0.743		

I = 'Individual' scores; Gr = 'Group' scores.

The next question to be answered was which of the two ratios da/ds or bs/ds was the more reliable one, for if the silences were more meaningful than the actions it seemed reasonable to conclude that the bs/ds ratio which disregards actions altogether should prove more sensitive a measure than the da/ds ratio. Or, in terms of our criterion, the

constants β_1 and β_2 should differentiate more significantly between the constant speakers and the partner groups for the bs/ds than for the da/ds ratios. Table 3 shows the result of calculating t -tests comparing the means of β_1 and β_2 of the seven constant speakers and their partner groups for both ratios.

From Table 3 we can see that on the average, at least in respect to skewness (β_1), the bs/ds ratio is the more sensitive measure.

The frequency of long silences thus shows a higher degree of reliability than any of the other ratio-components used in this investigation. A capacity for maintaining long periods of silence, or holding up action, or, viewed differently, an inhibition of expression in social interaction, seems to constitute a relatively permanent feature in an individual's conversational behaviour.

IX. SUMMARY

1. E. D. Chapple claimed that certain relationships emerging from the measurement of time sequences in social interaction, such as conversations and interviews, were significant for the assessment of personality. The assumption underlying these claims is that of the consistency of individuals in their conversational behaviour.

2. An experiment was undertaken to test the hypothesis that certain relations between time sequences in conversation were constant within limits for individuals and that homogeneity was maintained over and above the fluctuations due to changing situation, topics and partners.

3. Seven series of between nine to twelve conversations of a controversial character between seven constant speakers with the corresponding number (9-12) of people (i.e. seventy-one conversations altogether) were recorded on a chronograph tape with respect to the durations of actions (talk and gestures) and silences.

4. The ratio of long actions (da) over long silences (ds) was chosen to serve as the score measuring the constant and individually characteristic element in conversational activity. Scores derived from any of the seven constant speakers are termed 'individual' scores, scores derived from the groups of different partners are termed 'group' scores. 'Individual' scores would thus always come from the same person, 'group' scores from different people being the partners of the constant speakers.

5. As the frequency distribution of this score for the total experimental population was a J-shaped one, and the usual statistics comparing the variabilities of two groups were inapplicable, the method of moments was chosen to describe any differences between the distributions of the 'individual' as against the 'group' scores.

6. The mean β_1 (measure of skewness) for the distribution of individual scores was 0.489 (s.e. 0.141) and for groups 3.306 (s.e. 0.504). These results indicated that the score values coming from different people formed a more skewed distribution, whereas those coming from one and the same person were distributed with greater approach to symmetry. The mean β_2 (measure of kurtosis) for the individual score distributions was 1.931 (s.e. 0.234) and for groups 4.852 (s.e. 0.643) indicating flatness for the 'individual' score distributions and peakedness for the 'group' score distributions.

7. Taking each of the seven series of conversations separately the above figures are characteristic of six out of the seven series.

8. The question was asked as to the relative significance of the action or the silence pattern as measures of some reliability. Two ratios, one based exclusively on activity

(ba/da) and the other exclusively on silence (bs/ds) were computed and their sensitivity compared.

9. While the action ratio (ba/da) proved quite unreliable, the silence ratio (bs/ds) could be shown to be a consistent measure of conversational behaviour judging from the comparison of the shape of its distributions as described by the method of moments.

10. A comparison of the two ratios (da/ds and bs/ds) shows that the bs/ds ratio is the more sensitive measure, i.e. more limited in range for individuals and more dispersed for groups. This was taken to indicate that of the four classes of conversational behaviour investigated and defined by the frequency of short silences (bs), short actions (ba), long silences (ds) and long actions (da), the frequency of long silences is most characteristic of an individual's conversational activity.

11. In conclusion it may be said that the analysis of the data emerging from this experiment adds strong support to the hypothesis that certain relations of time sequences of action and silence in conversation tend to be constant within limits and characteristic of individuals independent of changing partners and topics.

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BINOCULAR FUSION OF COLOURS

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I. INTRODUCTION

In recent work on the possible existence of an independent receptor for yellow light, a number of methods were examined which produced in observers the sensation of yellow when red and green lights were presented binocularly.

The experiments reported in the literature show great individual variations in the ability of observers to fuse colours, and the results obtained seem in no small measure dependent on the experimental method used (Kries⁽¹⁵⁾, Hecht⁽¹⁴⁾, Johannsen⁽¹³⁾, Pickford⁽¹⁸⁾, Murray⁽¹⁷⁾, Prentice⁽¹⁹⁾). Hecht's (op. cit.) findings were criticized by Murray (op. cit.), who pointed out several pitfalls in his procedure. Most of these objections, however, have been experimentally disproved by Prentice (op. cit.).

In the present paper are reported several methods by means of which fusion was obtained.

II. EXPERIMENTS WITH HECHT'S FUSION BOX

The subjects used in these experiments were nine students of 20-30 years of age. Three of these were red-green defectives (as determined by the Ishihara test); the other six had normal colour vision. All of these nine subjects had been tested previously and were found to be unable to obtain yellow by fusion of red and green lights presented binocularly through the Fusion Box, and under the conditions described by Hecht (op. cit.).

The colour filters used in the following experiments were:

Ilford spectral filters no. 602 blue, transmitting from 4500 to 5000 Å.

Ilford spectral filters no. 604 green, transmitting from 5000 to 5400 Å.

Ilford spectral filters no. 606 yellow, transmitting from 5600 to 6100 Å.

Ilford spectral filters no. 608 red, transmitting from 6200 Å. into infra-red.

It can be seen that the filters employed have fairly narrow transmission bands, and that the overlap is small—the yellow filters transmit about 2% in the red and about 1% in the green part of the spectrum.

A slightly modified form of the Hecht Fusion Box was used, into which the part of a stereoscope, holding the lenses and Ilford spectral filters, was built. The subjects were asked to fixate a 250 W. pearl light bulb, mounted 1 m. away.

Of the nine subjects tested under the conditions described above, four reported seeing a well-saturated yellow square flanked on the one side by a green square and on the other by a red square. The others reported rivalry. These four subjects were then instructed—after adaptation—to fixate a white background. All of them reported seeing the negative

after-images, namely a violet square with a purple square on one side and a blue-green square on the other. The violet square could be made to disappear by closing one eye. Since none of the subjects was able to produce this violet after-image monocularly, it can be assumed that we are dealing here with the negative after-image of the fusion product of red and green.

When the five subjects who could not obtain fusion under the conditions described above were further tested, it was found that when the aperture through which the filters were used was reduced, all observers were able to obtain yellow by fusion. The best effect was achieved by using a visual angle of less than 2° , i.e. by foveal stimulation.

When blue and yellow Ilford spectral filters were used as stimuli they were instantly fused to white by the six normal observers. The three colour defectives reported seeing one half of the field as yellow, the other half as white.

These observations are not in agreement with Hecht's suggestion that there exists a relation between colour defect and failure to obtain fusion.

III. CONDITIONING EXPERIMENTS

Method. In these experiments eight observers were used; three of these were red-green defectives (as determined by the Ishihara test). When tested under the ordinary conditions of the Hecht Fusion Box, with stimuli of extra-foveal size, none of them was able to obtain yellow by fusion of red and green lights. Ilford spectral filters, as in the previous experiments, were employed.

The observer was seated comfortably in front of the unmodified Hecht Fusion Box, which was mounted on a wooden frame to avoid movement. The observer's chin was placed on a chin-rest, so that in this position the aperture of the Fusion Box was at the subject's eye level. The subject's arm was strapped to a solid table so that movement was not possible, and his hand was connected to the P.G.R. instrument. The observer was balanced in the apparatus in the usual way. Red, green and blue spectral light stimuli, as well as a series of different brightness levels of white light (produced by inserting Ilford neutral tint filters), were presented binocularly. Two to three units deflexion were recorded on the galvanometer in response to these stimuli, which were presented in random order and without any reinforcement until extinction of response occurred after approximately sixteen trials.

When it was definitely established that none of these spectral stimuli would produce any measurable response, conditioning of the subject to yellow spectral stimuli was begun by applying a slight electric shock from a battery-coupled induction coil to the observer's contralateral arm. Every time yellow light was presented binocularly galvanometer deflexions of 5-8 units were recorded. When after forty trials of yellow light and shock combination, the shock was omitted, binocular stimulation of yellow light alone produced a needle deflexion of more than 5 units. This response was taken as the criterion of conditioning, and was obtained each time yellow light was presented.

Test experiments and results. When red and green spectral lights were now presented binocularly to the subjects who were conditioned to yellow light, a galvanometer response was obtained which always fell well within the limits of the criterion accepted as a sign of conditioning (needle deflexion of 5 units and above).

It was interesting to note that, while all observers (including the red-green defectives) gave this positive conditioned needle response, none of them was actually experiencing

the sensation of yellow. When asked to report what they were seeing all reported seeing red and green rivalry.

Twenty trials were run in which the subject was asked to match red and green in brightness by means of neutral wedges. The same galvanometer response was given as in previous trials where the two stimulating lights were of different intensity.

Further series of experiments were carried out in which all the intensity levels employed with spectrum filters were reproduced by a combination of neutral tint filters. No galvanometer response was recorded. Further, in order to minimize the effects of chromatic adaptation, all colour filters were interchanged at each experimental session.

When red, green and blue light stimuli were presented singly, it was found that blue light did not produce a measurable response, while red light stimulation produced a response of 1 or 2 units. The same effect was observed when green light was used as a stimulus.

Remarks. Since positive responses to singly-presented light stimuli had been extinguished prior to conditioning to yellow light, the finding that a slight positive response is given to green as well as to red light presented singly, after conditioning to yellow, points to the assumption—postulated by most colour theories—that the red and the green receptors are sensitive to a fairly wide spectral range. It is possible, therefore, that they are involved when the eye is being conditioned to yellow light.

IV. INTERMITTENT STIMULATION EXPERIMENTS

(i) *Introduction*

In subjects unable to do so under the normal Hecht Fusion Box conditions, a further method of producing the sensation of yellow by fusing red and green light was found by using rhythmic instead of continuous stimulation.

This observation has been quantitatively investigated under two different conditions. In the first series of trials both eyes were stimulated simultaneously by flicker, while in the second series alternate stimulation of first one eye and then the other was used.

Thomas Young—as far back as 1812—called attention to the fact that the physiological correlate of a single flash of light was often not one but a series of pulsations; their number depending on the intensity and duration of the flash stimulus. This phenomenon he called ‘recurrent vision’.

When a disk having a slit along its radial axis is rotated at a particular frequency in front of a uniformly illuminated screen, certain coloured bands will be seen. These fluctuations were first described by Charpentier⁽⁶⁾, and are called ‘Charpentier’s bands’. The oscillations occurring at the end of the primary visual response have also been described by Bidwell⁽⁴⁾ and Ebbecke⁽⁹⁾. Bidwell states that at a frequency of stimulation of 6–8 per sec. the stimulation colour may almost appear to resemble that of its complementary. The change of colour with periodic stimuli is especially striking in the yellow-red part of the spectrum, which almost becomes green. These conclusions were confirmed by Burch⁽⁵⁾, Sherrington⁽²⁰⁾ and Allen⁽²⁾.

This last investigator, using monocular flicker stimulation, found that both inhibition and facilitation are at work, and from his binocular experiments (presenting flicker to one eye and a constant coloured light to the other) he showed the existence of a trichromatic subdivision of the spectrum. Any of the three primaries presented to one eye

increases sensitivity to the corresponding colour in the other, the wave-lengths 0.655 and 0.500 μ m. being invariable.

It was interesting to re-examine the findings of these investigators, and to ascertain some of the conditions which may affect binocular fusion of colours when the eye is subjected to rhythmic stimulation with coloured lights.

Procedure. Two light sources—having a 36 W. 12 V. car lamp each—were used and well shielded from each other. In front of them were mounted the required colour filters (Ilford spectrum red transmitting between 6100 Å. and infra-red and Ilford spectrum green transmitting between 5000 and 5450 Å.). The red filter does not transmit more than 2%, the green filter less than 1% in the yellow part of the spectrum. The graduated optical wedges used could be adjusted by the subject.

Mounted to the shaft of a motor was a disk which covered the whole of the visual field. Into this disk two slits had been cut, which subtended the same visual angle, so that the eyes could be stimulated intermittently. The slits were adjusted in different disks so that either simultaneous stimulation of both eyes or alternate stimulation of first one eye and then the other could be achieved in different series of experiments. The frequency of stimulation could be adjusted by the subject by means of a rheostat connected to the motor. The size of the field could be varied by attaching black paper frames to the stimulus panels. The overall illumination was produced by a 100 W. pearl bulb.

(ii) *Rhythmic stimulation of both eyes simultaneously*

The subjects observed the stimulus panels through a blackened box, similar in structure to that employed by Hecht, but having an open 4 × 4 in. front.

The subject placed his chin on a chin-rest, and by means of the neutral wedge matched the brightness of both stimulus panels, which were illuminated with red and green light (Ilford spectral filters) respectively.

The mean brightness level was 12 f.c., and the visual angle subtended was 10°. The observer was then asked to focus a distant object so that superimposition of both coloured stimuli was achieved. Then the subject started the motor and adjusted the speed of the sectored disk by means of the rheostat. For each condition of illumination and field size, ten readings were taken at predetermined numbers of revolutions.

Eight subjects were used, six males and two females. One subject was about 50 years of age, the others were between the ages of 20 and 30. Five of the six were students, of whom one had been turned down by the Air Force for defective colour vision. It was interesting to note that his reactions did not differ from the mean result of those with normal colour vision. (Five of the subjects had been used in previous experiments on fusion, and were found to be unable to obtain it under the conditions described above. Those five subjects, however, gave a positive response to red and green stimulation when the 'skin reflex method' described above was employed, although they reported seeing, in fact, rivalry.)

Results and discussion. None of the observers tested in this experiment could obtain yellow fusion in any way when the stimulus panels were under constant illumination. Even continuous stimulation up to 150 sec. did not produce it, nor could any method of focusing.

When the sectored disk was interposed between the observer's eye and the light source, and both eyes were stimulated simultaneously by flicker at a mean rotation of about

5.5 rev./sec., the sensation of yellow was experienced and became most saturated at about 10 rev./sec.

The effect was greatly enhanced by focusing a distant object. Above a speed of about 8 rev./sec.—and while yellow fusion still persisted—the observers reported the sensation of subjective colour phenomena, which amply confirms the findings of Bidwell(4), Fechner(11) and others.

Near the critical flicker frequency of the sector these subjective colours disappeared, and the perception of yellow was replaced by the colour of the stimulating wave-length. It is to be noted that at this stage green, not red, supervened. Retinal rivalry proper again became manifest at frequencies above the critical flicker frequency.

Table 1. *Effect of intensity and visual angle on fusion with simultaneous stimulation*

Intensity of stimulus (f.c.)	Visual angle subtended (°)	Stimulus frequency to obtain fusion
12	10	9.8
7	10	6.5
5	10	3.9
12	5	6.6
7	5	6.0
5	5	4.6
12	3	4.9
7	3	4.1
5	3	4.0

Table 2. *Effect of intensity and visual angle on fusion with alternate stimulation*

Intensity of stimulus (f.c.)	Visual angle subtended (°)	Stimulus frequency to obtain fusion
12	10	12.5
7	10	10.1
5	10	8.9
12	5	9.0
7	5	8.4
5	5	8.2
12	3	8.5
7	3	8.1
5	3	8.0

It can therefore be seen that there is a certain range of frequency at which periodic stimulation has a reliable effect in producing binocular yellow, as well as other subjective phenomena.

The effect of intensity and visual angle. The effect of stimulus intensity and stimulus size on this critical flicker frequency for the sensation of yellow has been investigated, and it was found that the fusion threshold for yellow diminished with decrease in the visual angle subtended by the stimulus. Thus, when a 10° field was used the threshold was higher than with a 6° field. The threshold was lowest with the smallest visual angle of 3° (Table 1).

When the stimulus size was held constant and the stimulus intensity was varied, it was found that at low brightness levels the fusion threshold for yellow was low. (This finding contrasts with those of recent investigators who, using continuous stimulation, report that high levels of illumination are needed for fusion of yellow to occur.)

The fusion threshold increased with increase in illumination, and this is fully in accord with the findings of Bartley(3), Lythgoe(16) and others, who state that the subjective

fusion frequency for the human eye increases with an increase in the intensity of the stimulus. Creed & Granit(7) and Adrian & Mathews(1) have shown that this relation also holds when the fusion frequency is recorded electrically from retina and optic nerve.

(iii) *Rhythmic stimulation of both eyes alternately*

These experiments were run in exactly the same way as those described above (where both eyes were stimulated simultaneously).

Result. It was found that yellow fusion could be obtained, but that its threshold was higher and its saturation was less than that when both eyes were stimulated together. The dependence of fusion on intensity of stimulation as well as on field size showed a strikingly similar relation to that discussed under conditions of simultaneous stimulation of both eyes.

Table 2 gives the influence of visual angle and stimulus intensity on frequency of stimulation to obtain yellow fusion when both eyes are stimulated alternately. If this table be compared with Table 1 it can be seen that under the same conditions of visual angle and intensity the frequency of stimulation to obtain fusion is lower by simultaneous than by alternate stimulation.

V. STATISTICAL ANALYSIS AND DISCUSSION

The foregoing impressions of the effect of intensity, visual angle, and the two methods of stimulation (i.e. simultaneous and alternate) are confirmed by the application of

Table 3. *Frequencies (rev./sec.) required for fusion*

Visual angle (°)	Method A. Simultaneous stimulation. Intensity (f.c.)			Method B. Alternate stimulation. Intensity (f.c.)		
	Intensity (f.c.)			Intensity (f.c.)		
	5	7	12	5	7	12
3	4.0	4.1	4.9	8.0	8.1	8.5
5	4.6	6.0	6.6	8.2	8.4	9.0
10	3.9	6.5	9.8	8.9	10.1	12.5

Explanation in text. The figures given here are the same as those in Tables 1 and 2, but are re-arranged for statistical treatment.

Table 4. *Analysis of variance*

Source of variation	Mean square	Degrees of freedom
Due to intensity	7.91	2
Due to visual angle	8.47	2
Due to method	54.43	1
Intensity $\times$ angle	2.40	4
Intensity $\times$ method	0.66	2
Angle $\times$ method	0.52	2
Intensity $\times$ angle $\times$ method	0.14	4
		17

analysis of variance technique to the data. The data on which the analysis is based are given in Table 3, and the resulting analysis of variance is summarized in Table 4. Various mean 'fusion frequencies', corresponding to different levels of intensity and visual angle, are listed in Table 5.

It is evident from a study of Table 4 that the separate effects due to intensity, visual angle and the stimulation method are all highly significant. In particular, the observed variation due to differences between methods of stimulation is very marked, and there

is less than 1 chance in 1000 of observing such a variation under an hypothesis of no real difference between methods. It can be readily concluded that the method of alternate stimulation results in a greater fusion frequency than the method of simultaneous stimulation.

In addition to the separate effects of these three factors there is also a marked interaction effect between intensity and visual angle. This implies that either the effect of

Table 5. *Mean frequencies required for fusion*

	Rev./sec.
Mean frequency under simultaneous stimulation	5.6
Mean frequency under alternate stimulation	9.1
Mean frequency at visual angle 3°	6.3
Mean frequency at visual angle 5°	7.1
Mean frequency at visual angle 10°	8.6
Mean frequency at intensity 5 f.c.	6.3
Mean frequency at intensity 7 f.c.	7.2
Mean frequency at intensity 12 f.c.	8.6
Mean frequency over all angles, intensities and both methods	7.3

intensity is modified by visual angle such that the increase in the fusion frequency with increasing intensity is much more pronounced at larger visual angles or, alternatively, the effect of visual angle is modified by intensity such that the increase in fusion frequency with increasing visual angle is more marked at the greater intensities. It may be noted that the variation in frequency induced by varying the visual angle between 3 and 10° is approximately the same as the variation resulting from varying the intensity between 5 and 12 f.c.

VI. FURTHER OBSERVATIONS

One fact which showed up strikingly under all the observed conditions was the great variability around the fusion threshold. It often occurred, for instance, that the subject reported optimal yellow fusion at different frequencies in the same series, which showed up at different frequencies of stimulation when the series was repeated. Even more marked was this threshold variance if the frequency was held constant at a point when the subject reported seeing 'very saturated yellow'. Often this clear sensation faded and came back, faded again, etc. This occurred at almost any frequency level which was observed for more than 25 sec. It is proposed to investigate this threshold variation quantitatively in a later study.

Since the primary interest in this study was to explore the conditions which produce yellow fusion, the main emphasis has been placed on differences between simultaneous and alternating stimulation of both eyes by flicker. Other factors, though certainly of importance, have not been investigated fully, nor has any attempt been made to correlate the present findings with those obtained by other workers who employed black and white flicker stimulation.

Differences in intensity between red and green light stimuli produced no significant effect on yellow fusion. In order to minimize possible effects of adaptation or fatigue, both colour filters were interchanged in random order at each experiment. It was found that in the first few seconds after each change the resulting yellow fusion was less saturated. Maximum saturation was reported generally within 20 sec. After this time fluctuations occurred again.

The writer has found that subjects experience great difficulty in obtaining fusion with continuous light stimulation under the conditions of this experiment, while fusion was readily obtained on employing flicker stimulation (either simultaneously to both eyes or alternately—first to one and then to the other eye in quick succession). Allen⁽²⁾ has interpreted his findings by assuming processes of facilitation and inhibition to take place when the eye is stimulated by flicker. Thus, when one eye is stimulated by one colour it inhibits the response of the other and rivalry results.

It will be seen from the results of the present investigation that retinal rivalry is the rule when the stimulus frequency is small, and that at low levels of illumination it is difficult to find any very definite frequency of stimulation which gives rise to a really saturated yellow fusion product. Such a yellow has always been obtained easily and at definite frequencies when the stimulus intensity was increased.

This finding seems to point to the assumption that each sensation has its maximum value, and if it is not prevented from reaching this value by the next incoming stimulus the subject is able to find a flicker frequency which will give him the most saturated fusion product.

Low stimulus intensity, however, will reach this maximum value more slowly and will almost always be prevented from reaching it by the next incoming stimulus, so that at low levels of stimulus intensity the fusion product is never seen to be fully saturated. If these low intensity stimuli were allowed to reach their maximum value this frequency would have to be so small that rivalry would result instead of fusion. Greater visual fields, probably on account of summation, have been found to demand a greater stimulus frequency for fusion to occur.

Although Hecht's curves indicate fusion to occur first in the periphery, the implications of this are by no means clear, and are unlikely to offer an explanation for the results under discussion.

VII. GENERAL DISCUSSION

In all these methods in which instant fusion was obtained, especially in the last one where intermittent stimulation was employed, it was remarkable to observe the suddenness with which fusion occurred and the ease with which it could be obtained in every subject, including the red-green defectives, who can always see yellow. This latter fact may be difficult to account for by means of a purely trichromatic theory, and it has led some workers (such as Fick⁽¹²⁾) to regard the green response curve of deuteranopes as shifted towards the red end of the spectrum and the red curve in protanopes as shifted towards the green. Walls⁽²¹⁾ gives a good account of it. On the whole, however, as suggested by Pickford⁽¹⁸⁾, a theory postulating more than three receptors can account for these facts more easily.

However, the difficulty in explaining the ability of red and green defectives to fuse red and green into yellow may be eased if one assumes, as does de Vries⁽⁸⁾, interconnections between receptor mechanisms so that red *receptors*, for instance, may be linked to green *mechanisms*. This arrangement would lead to a 'normal response' in colour *mixing* experiments. (Colour *discrimination* tests, however, show a sensitivity curve of receptors which differs from the sensitivity curve of the mechanism.)

It must, however, be borne in mind that a distinction exists between colour *receptors* and colour *sensations*. The one may, indeed, have little to do with the other. A red

receptor is one which is most sensitive to the red wave-length, whereas the sensation of red will result when all the receptors are stimulated, but the red more than the others.

In all these experiments where colour defectives are used as observers it is most important to start with an accurate diagnosis of the subject's type of defect, and some of the differences of opinion among workers in this field in explaining their data may be found to resolve themselves as soon as clear-cut diagnostic aids become available, and the same standards universally applied.

VIII. SUMMARY

The following points emerge from the above investigations:

(1) Immediate binocular fusion of colours has been found to result on using appropriate methods, three of which are discussed in this paper.

(2) Yellow fusion could be obtained with ease in all subjects by using fields of less than 2° ; conditioning to yellow light, using the galvanometer deflexion of the P.G.R. as an index; and by rhythmic binocular stimulation.

(3) The effect on yellow fusion of binocular stimulation with red and green lights has been investigated quantitatively by stimulating both eyes simultaneously; and by stimulating both eyes alternately.

With this method, special regions of stimulus frequency were found which gave optimal fusion effects. The effect of stimulus size and stimulus intensity on these special stimulus frequencies was determined, and it was found that increase in both these variables had the effect of causing a rise in the frequency of stimulation at which colour fusion occurred. It was also found that under equal conditions alternate stimulation of the eyes required a higher stimulus frequency than simultaneous stimulation. Differences in the intensity of the two stimulating lights have been found to be of no importance in obtaining fusion by the methods described. The possible significance of the finding that yellow fusion was also experienced by the colour defective observers is discussed.

The writer gratefully acknowledges the helpful discussions so freely given by his colleagues at the Vision Research Unit, and by Dr R. W. Pickford.

Thanks are due to Dr A. Lawn for his help in the design and construction of the recording apparatus used in the conditioning experiment, and to Dr D. Westgarth for his advice on the statistical methods employed.

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CRITICAL NOTICE

BY AGNES CRAWFORD

Individual Differences in Colour Vision. BY R. W. PICKFORD. London: Routledge and Kegan Paul. 1951. Pp. xviii+386. 35s. net.

Dr Pickford's book has been long awaited as the first book on this topic by a psychologist since Dr Mary Collins's study of the colour vision of a small number of colour-blind people in 1924. This new book differs from its predecessors in two points at least: first, it deals with the colour vision of a large number of subjects, both with normal and with abnormal colour vision; and, secondly, its author is concerned, as Dr Collins was not, to put forward and support with experimental and statistical evidence, a theory of colour vision that will explain all the facts.

The plan of the book is simple. There is an introductory chapter, dealing briefly with a number of topics, including 'stimulus, receptor and sensation'. Accepting von Kries's Duplicity Theory, and experimenting with cone vision, Dr Pickford does not spend time on descriptions of retinal structure; he outlines the effects of colour mixing both with reflected and transmitted lights, and discusses at some length the trichromatic equations established by W. D. Wright, to whose work he pays tribute. He disagrees with Wright, however, on the theory based on these trichromatic equations, and maintains that instead of showing that only three receptors are necessary for the perception of colour, the equations show that at least four receptors are necessary. From this, Dr Pickford goes on to outline four of the main theories of colour vision. He dismisses Ladd-Franklin and Edridge-Green's theories, and in his discussion of the trichromatic equations has already shown that the Young-Helmholtz theory does not explain the observed facts of colour vision defect. He states the Hering theory of colour vision, with the modifications suggested by R. A. Houstoun and suggests further modifications. He discusses this more fully in his final chapter when he has the evidence of his own experimental work, and of his factor analyses of the results to add to some suggestive experimental work by Granit and others; all of which seem to support his modification of the Hering theory.

In this first chapter, he outlines briefly the work done in testing the development of colour vision in children. He collects the evidence about the frequency of colour-vision defect among European and other populations, and then goes on to describe how he gradually developed his own apparatus and his testing method, beginning with the standard Rayleigh equation obtained by mixing colours from rotating disks, and later, because of the difficulties he found with this, developed his own anomaloscope. He used this apparatus for a variety of tests: the 'paired filters' test, a 'spectrum filters' test, a 'nine-colour experiment', an experiment in matching intermediate colours, on to the final version of his test, which he calls the 'four-colour test'. The account of this development and refinement of the apparatus and the testing technique is interesting and at times amusing. It left one with the feeling that for Dr Pickford the whole of his experimental work could well be described as 'adventures in colour-vision testing'.

From all these experiments Dr Pickford has taken two sets of measurements: the mid-matching point, and the double differential threshold (or range). The former he is not interested in as a figure, but as a deviation from the average mid-matching point of a group of subjects with normal colour vision: the latter he is interested in in relation to the modal range for normal subjects. There is a third measured variable, the brightness of the standard colour at which matches were made. This was adjusted as required for each person tested, and was nearly constant for those with normal colour vision, though it varied considerably for those with abnormal colour vision, and was indeed the chief way of distinguishing protanopes from deuteranopes. The four-colour test shows also that protanopes seem to see the spectrum green as brighter than the standard yellow, while for the deuteranopes the standard yellow, the spectrum red and the spectrum green are all of equal brightness, as they are for normals. Besides the four generally recognized groups of colour blind—protanopes, deuteranopes, and red and green anomalous—Dr Pickford has found some subgroups. He maintains that there is a subgroup of red-anomalous, for whom the red is not darkened, and a subgroup of deuteranopes who have much greater defect in green than in red (deviant deuteranopes, he calls these), and a further group of 'red-green minor defects'. This last group may be continuous with the normal distribution of colour vision, but the first six groups are quite distinct from the normal distribution. Besides this large group of red-green defects, there is a group of

people with yellow-blue defects, divided into (a) yellow-blue deviants (anomalous matching points) and (b) yellow-blue weakness, which shows as wide matching ranges. This last shows two forms, one with and one without darkened violet.

In contrast to the red-green defects, for which the inheritance seems to be clearly sex-linked, the yellow-blue defect does not seem to be sex linked, and, according to Dr Pickford, is more frequent than is generally supposed: the low frequency usually quoted is, in his opinion, mainly due to lack of an adequate test: a lack which has now been made good by Dr Pickford's yellow-blue test.

One of the most valuable contributions by Dr Pickford in this comprehensive study is his factor analyses of the correlations between the test measurements. He has carried out factor analyses for every one of his experiments, dealing separately with the measurements from normal people and those from people with defective colour vision. Some of his studies are based on the correlations of very small numbers of test results, but the same sort of factors with the same general level of saturations are obtained, with both large and small groups, and with both normal and abnormal groups. He finds a general factor, which he defines as ability to do the test at all and then two bi-polar factors: a red-green factor and a blue-yellow factor. As separate factors, these are of course independent of each other; Dr Pickford interprets this as evidence in favour of yellow as an independent primary receptor, separate from red or green. Putting this factorial evidence along with his experimental results and the theoretical possibilities about the functioning of cones, Dr Pickford elaborates Houstoun's modification of Hering's theory to suggest that there may be either (a) two sets of two cones, one set responding to red and green, and the other to yellow and blue, or (b) two sorts of cones, each responding in different ways to the two pairs of colours. He prefers the former to the latter. With either, there is also a separate cone receptor for brightness, largely independent of the colour cones. He discusses the kinds of defect which could arise from such an arrangement of cones, and finds that they agree very well with the sorts of defects that he has found in his experimental groups. He points out that the bi-polarity of red and green, and of blue and yellow is not due to the experimental technique, though with most anomaloscopes, the addition of green to a mixture of red and green means also that the amount of red in the mixture is reduced. I am not entirely convinced by the arguments he brings forward on this point, and would like to suggest that it might be possible so to arrange the apparatus that, in the Rayleigh equation for example, the amount of red light was kept constant, and green added until the mixture matched the standard yellow. To do this, it would be necessary to be able to increase the brightness of the standard yellow as green, and extra light, was added to the variable, or else to begin with the yellow brighter than the red. If this could be done, the negative correlation between amount of red and amount of green in the mixture could be got rid of experimentally instead of statistically, and we could be more sure that the bi-polarity was not an artefact of the experimental technique.

There is one other point of experimental technique on which I disagree with Dr Pickford. He takes one or two, sometimes two or three, sets of measurements from each subject. While I have much less experience of this sort of testing than he has, I do not find so small a number of measurements satisfactory, and, in fact, find that the first set of measurements is significantly different from the second, even although all the subjects have had a preliminary trial run. With colour-blind subjects, a set of ten readings shows that some vary greatly in their matching points from one trial to another, while others do not. Most normal subjects show very little variation after the first recorded trial, but those whose measurements are about two or three sigmas from the mean for normals vary more in their judgements than those nearer the average. However, few of the people who have worked with this sort of colour-vision test seem to think it necessary to take as many as ten measurements from each subject, and to do so does add considerably to the time taken for administering the test: an important practical disadvantage.

Besides the points I have hitherto mentioned—the careful experimental work, the factor analyses—there are throughout the book extremely interesting descriptions of the confusions and mistakes in colour-naming made by the numerous colour-blind subjects. Sir Frederic Bartlett stresses the value of these in his foreword to the book, and also points out the usefulness of Dr Pickford's descriptions of the other cues which colour-blind people use to conceal their defect. In the final chapter, Dr Pickford discusses the possible genetic mechanism for the inheritance of colour blindness. He deals rather harshly with the Ishihara test, which he describes as completely ineffective. He thinks that the Four-colour test could usefully replace the Ishihara. I agree with this, but am not quite so sure that it is as easy for children to do this test accurately, or for test-naïve adults to do it accurately, as he seems to think, though relatively little practice is needed to obtain accurate judgements.

These are but minor criticisms, however, and do not detract greatly from a book which enables us to share Dr Pickford's enthusiasm for his research, and to benefit from his skill in testing subjects who are often resistant.

CRITICAL NOTICE

By GEORGE SETH

Psychology and its Bearing on Education. By C. W. VALENTINE. London: Macmillan. 1950. Pp. xviii + 674. 18s. net.

A new basic text on psychology for teachers in training has been long overdue. In his latest book Prof. Valentine has hoped to fill this gap. At the same time he has intended to provide an introduction to psychology not only for youth leaders and others who are directly concerned with education, but also for social workers in other fields. His fear that the attempt would be open to the criticism of being too general for the teachers was unfounded, but despite the breadth of his sweep, it may be too educational in its emphasis for the others at whom it is directed.

His principal aims are explicitly stated in the preface. The first of these was clarity: for the most part he achieves this except for some sentences where the colloquialism or the discursiveness of the writing tends to obscurity, and the far too frequent occasions when his proof readers have failed him. Secondly, he has tried to make the early chapters 'as simple and general as possible', returning later in cyclic fashion to deal with the same topics in a more detailed way. This deliberate technique takes account of the variety of readers whose needs may be very different and whose range of competence very wide. But it leads to some repetition, which has added unnecessarily to the length of an already long book. Thirdly, he succeeds in avoiding controversy with which the elementary student should not be confused. Fourthly, he achieves comprehensiveness, though he hardly does so well—but can we blame him for this?—in avoiding the suspicion of superficiality. There can be few general psychological issues about which the young teacher ought to ask questions that do not somewhere find a place, or indeed several illuminatingly different places, in Prof. Valentine's thirty-six chapters. But one could wish that, for the graduate student at least, some of his answers had been more thoroughly argued. He sets out to interest the student in the problems of human nature, and not merely in children, or pupils, or classroom techniques. In doing so he recognizes that the teacher, too, is a human being, who may not unreasonably look to his instructors in psychology for some assistance towards self-awareness and for some light upon his personal problems.

The chapters of the book fall into four broad groups, not separated into sections. The first of these deals in a general way with the problems of human ability and motivation. The discussion of the former follows the lines of current factor theory: the latter is broadly based on the psychological doctrine of instinct, although it is less narrowly McDougall than the chapter-headings suggest. The second takes up more specifically the bearings of psychology upon educational practice. In this section, but somewhat oddly placed at the end of a discussion of problems of educational and vocational guidance, come three important chapters on 'The Appreciation of Beauty and Aesthetic Education', a topic which, despite its relevance to the education of the emotions, has been much neglected in psychological discussion. Relying in these very largely upon a long series of his own investigations, Prof. Valentine discusses in turn (1) Nature and Visual Art, (2) Music and (3) Poetry. The third broad grouping covers a discussion of the problems and techniques—tests, record-cards, and so forth—of educational and vocational guidance and selection. It includes an account of the Quota scheme for grammar-school selection already recommended by Prof. Valentine, and successfully applied in Walsall and elsewhere. In the course of his discussion, he takes an optimistic view of the effect of the introduction of intelligence tests, as such, upon 'cramming' for secondary-school entrance. In this matter, at least, the Northern Ireland leopard, with whom Prof. Valentine is not unacquainted, has not changed his spots. The case is perhaps not so different elsewhere. If it were only that it offers some hope of obviating this bogey of the primary school, the more widespread use of the Quota scheme would have much to commend it. The last series of chapters deals with the problems of child development from infancy through adolescence.

On none of these points is the discussion colourless or merely academic. Throughout the book the marks of Prof. Valentine's idiosyncrasy are in evidence. The first of these is his conviction, shared to the full by the reviewer, that the teacher and social worker should not learn first, nor merely, an 'applied' psychology. In this matter his practice accords with his precept, and the point is the more significant when it is made by one who, over a long span of university teaching, has done as much as any educationist in this country to make psychological principle effective in the everyday work of the school. While he believes that

adjusting the school to the child is an ideal worthy of the best efforts of the psychologist, he is properly impatient of extremes and slogans—'Freedom in Education', 'The Play Method', and the like—as tending to defeat themselves. The most characteristically personal aspect of the book is to be found, as we might expect, in the fact that more than any other writer in England or America he has brought to the psychology of education a wealth of human understanding and psychological observation of the process of normal child development in all its aspects. Finally, there runs through the whole discussion his interest in what is sometimes called mental health and in the contribution that education, whether broadly or narrowly conceived, can make to the personal social adjustment of the growing individual. Relatively little space is given to problems of *maladjustment* as such, and the book is disappointingly superficial in its treatment of 'Problem Children' and young delinquents. But it should be acknowledged that it does not set out to deal with 'sectional' problems, and the sanity and wisdom of its comment on the ordinary problems of the ordinary child, and on parent-child and teacher-child relations are particularly sound and realistic. They provide a secure foundation for the spirit of tempered confidence in which one should like to see parent and teacher alike approach the handling of the child.

It is just over fifty years since William James published his *Talks to Teachers*. Prof. Valentine's book has much of the pleasant informality implied by James's title, but its size bears witness to the quantity of factual and research material over which his conversations had to range. At times it reads like a history-in-autobiography of educational psychology (and of many years of research in the Department of Education at Birmingham); it will be no less interesting to many of its readers because of that. The length of its span, in terms of Prof. Valentine's own researches, extends at least as far as investigations into the psychology of aesthetics carried out while he was a member of Stout's department in St Andrews in 1912-13. The book is important because it brings together, as no other has hitherto done, the results of the very considerable body of useful research in the psychological aspects of education carried out in an English setting over half a century. How much experiment there has been in this field, and how valuable much of it is, we often fail to recognize, although readers of the *British Journal of Educational Psychology* cannot be ignorant of it. For a great deal of that work Prof. Valentine has been himself directly responsible; he has been the inspiration of much of the rest.

It is easy in a book, whose range is so extensive, to find points which the reader with specialist interests would regard as having been inadequately treated. But such criticism makes a demand upon the author that he has not pretended to meet. Some surprising gaps, however, are apparent and at some points, for a book published in 1950, the discussion seems to be unjustifiably slight or unduly restricted. There is virtually no reference to Gestalt, and Thorndike is mentioned only twice. Nothing is said about the therapeutic possibilities of play—a surprising omission, despite the extravagance of some of the theorists, in view of Prof. Valentine's own emphasis on the 'mental hygiene' aspects of education, and his long-standing interest in aesthetics, with which the clinical theory of play, if we may call it that, has obvious links. It is surely inadequate in discussing the transfer of training to refer only to the investigations of Meredith and E. E. White, and in dealing with possible temperamental factors in mathematical work to notice only Alexander's 'X'. On general grounds, supported by some clinical suggestion and a number of hints, if no very satisfactory evidence in the literature, we should imagine that any such temperamental influences in mathematical study ran far deeper than any mere 'will to succeed' or 'persistence'. The latter, indeed, despite its pedagogical respectability, is a characteristic whose absence, even when it does not amount to positive 'laziness' or aversion, nevertheless begs a psychological question which is not likely to be answered by group tests. It is surprising, also, in spite of now not-so-recent legislation and consequential regulations, to find that Prof. Valentine continues to refer to the child whose i.q. is below 70 as mentally defective.

The book is liberally bespattered with typographical errors, and some irritating textual slips which should be corrected on reprinting.

PUBLICATIONS RECENTLY RECEIVED

Educational Psychology. BY WILLIAM CLARK TROW. Cambridge, Mass.: Houghton Mifflin Company (London: Allen and Unwin). Second edition, 1951. Pp. x+761. 35s. net.

This is a second edition of the book published under the same title in 1931 by the Professor of Educational Psychology in the University of Michigan. The earlier edition was not available to the reviewer, but it is clear from the range of the references that the extension has been considerable. On the showing of the author's preface, the emphasis on 'pupil-adjustment' which characterizes the general point of view from which the book is written has led to a fuller consideration of the social psychology of education in chapters on the influence of the environmental structure. There have also been added chapters on motor learning and aesthetic learning.

The British reader is likely to feel that the term educational psychology has been unduly stretched to include a quantity of material which, although certainly relevant in any programme of teacher-training, seems to fall outside the scope of educational psychology as such. The applied scientist must, of course, be allowed to offer his prescription. But some of Prof. Trow's material would find a more natural place in a discussion of educational philosophy, or educational principles, or even of ethics. Much of it is germane rather to a course on General Methods of Teaching, sometimes indeed on Special Method. It may be agreed that the foundations of method are psychological, and it may be argued that we would do well to try to clarify, more fully than we have yet done, the psychological rationale of many accepted classroom techniques. Prof. Trow would doubtless appeal to 'integration' in defence of his extremely comprehensive sweep, but he does not achieve an entirely satisfying coherence between his psychological material and his educational conclusions in which the one would appear to be the inevitable consequence of the other. The attempt to bring so much of the content of teacher-training within the framework of educational psychology seems to provide a suitable rejoinder to the view of a number of people in this country, which was given a kind of official blessing some years ago by the Norwood Report, that psychology should not be taught as such to the teacher in training, but should appear as an incident (if not an accident) of a more general course on educational theory. Rather Prof. Trow would hold that psychology is the natural and proper groundwork of many, if not all, of the more specialized aspects of the teacher's training.

He defines educational psychology as 'the study of the application of individual and social psychology as subject-matter, research techniques, and professional services to human development in a controlled environment'. He holds the reasonable view that psychological principles and theory derived from laboratory experiment cannot be taken over without modification into the educational sphere. Yet his own unadulterated borrowings of this kind are very considerable. There has, indeed, gone to the preparation of this new edition of his book a real attempt to overcome the time-lag of an applied psychology and to seek in recent psychological experiment for illumination of the educational problems he considers. But Prof. Trow has not been able always to get the light to fall exactly where he wanted it, so that the strictly psychological sections of the book and the discussion of educational questions often remain, so far as connected argument goes, separate and distinct. His chapter on motor learning does something to offset what Cole has called 'the neglect of the effectors', but for the most part his newer psychological material does not lead to any strikingly novel insights in his educational recommendations. It is not, of course, implied that in this he is either wrong or illogical. The level of the writing, even in its more narrowly psychological aspects, varies widely. The chapters on tests and testing, for example, are elementary by comparison with those on the learning process. References to British work are as usual conspicuous by their absence. It seems to be particularly unfortunate that he should quote the English law of 1534 and the British Royal Commission of 1904—the former admittedly for the reader's entertainment—on the subject of mental defect, while making no reference whatever to the significance of the recent statutory development of the conception of *educational subnormality*.

The book is always clear and full of sound practical suggestion, most of it relevant to education in any setting, whether American or English. It is humane and it is pleasantly written. It is well produced and it is adequately indexed. But it is too long.

G. SETH

Psychology in the Service of the School. BY M. F. CLEUGH. London: Methuen. 1951.

Pp. viii + 183. 7s. 6d. net.

A book of this type has long been needed and many teachers will find it of very real value. It is written in simple, non-technical language which makes it easy for the youngest student to understand, although its very simplicity may detract from its value in some instances. Some may find two things about its style irritating; the constant use of long brackets where footnotes seem the more obvious way of clarifying a statement (although footnotes are used in a few instances), and the driving home of points which are already perfectly clear.

The introduction is, perhaps, almost too reasonable and makes one wish that the author would definitely take a stand on some issue, nevertheless, even here there are important observations made and both teachers and clinical workers could profit from the general feeling for co-operation which emerges. This is stressed throughout the book and considerable space is given to the relative roles of the teacher and the various welfare organizations which are at his service. Although it is good for teachers to know that psychologists appreciate the often sterling worth of those who have no psychological knowledge and no faith in psychological help, it is doubtful whether quite so much stress need have been laid on the usefulness of such teachers, especially in view of the strong plea for early and correct diagnosis on the part of the school and its co-operation with the Child Guidance Centre or welfare organization. It is difficult to see how this can be successful without some sympathetic understanding of psychological methods.

In the main chapters of the book, however, this attitude is not so apparent. Chapter III gives a sound, simple explanation of maladjustment which should be acceptable to teacher and psychologist alike, and with chapter VIII, 'A Practical Guide to Action', should prove of very real value to head teachers who have the welfare of their pupils as their first consideration.

The many examples of cases mentioned in the text are often useful and illuminating but the seventy short examples in chapter VII are of doubtful value, with a few exceptions, as the information supplied is so meagre. Both the examples and the explanations in chapter IV on 'Fight and Flight' are possibly the most useful in the book, for few teachers have a deep understanding of the difficulties of the withdrawn child who causes no troubles in school and yet has, possibly, more maladjustment than the aggressive child who is a constant nuisance. The discussion of discipline in chapter V is also good.

Undoubtedly, this is a book which will be found useful not only by teachers actually in schools but by training departments of universities and colleges.

WINIFRED LANGAN

Aufgabe der Psychologie. Eine Geschichte ihrer Probleme. BY PAUL v. SCHILLER. Vienna:

Springer-Verlag. 1948. Pp. 233. 20s. net.

It is of interest and profit every now and again to examine our origins. The author of this volume on the task of psychology sees its beginnings, not in the laboratory of Wundt but in the pre-experimental reflexions of the philosophers from Plato and Aristotle, Augustine and Thomas Aquinas, onwards. He is at pains to emphasize the dualism that has been evident in psychology's methods and aims, turning on the one hand to behaviour as studied by observation, on the other to experience as studied by introspection. Not surprisingly, he devotes considerable attention to the schools of Gestalt and dynamic psychology, by which he is much influenced. Recognizing, as he does, the kinship between the theoretical problems which have engaged the attention of psychologists in the ancient world, in the Middle Ages, and to-day, he sees the diverse threads now woven together in the need to determine the laws of motivation, on which both the biology and the ethics of conduct depend and the elucidation of which must be regarded as the prime task of psychology.

Because of its orderly sequence and synoptic viewpoint this is an essay to be read through rather than a work of reference to be consulted for specific information.

E.O.M.

The Clairvoyant Theory of Perception: A New Theory of Vision. BY M. M. MONCRIEFF.

With a Foreword by H. H. PRICE. London: Faber and Faber Ltd. 1951. Pp. 315. 21s. net.

If we seek to reject extra-sensory perception we must show how to explain in other ways the phenomena supported by thousands of well-attested observations made before scientific experiments were carried out on the subject, and the vast odds which have been compiled in favour of extra-sensory perception since well-conducted experiments were made upon it. If, like the author of this book and the present reviewer, we are disposed to accept extra-sensory perception, it becomes interesting to consider how it

might fit in with current psychological and philosophical ideas. One approach to this problem would be to see how far the phenomena of extra-sensory perception might be explained by elaborations of familiar sensory and perceptual theories and concepts in physiology and psychology. Most people who attempt this course are inclined to think that extra-sensory perception would force them to adopt non-materialistic ideas and therefore regard it as conflicting with a materialistic standpoint. This seems likely, though it seems even more likely that materialism could not be supported on any grounds. On the other hand, it is always a possibility to be kept in mind that convincing materialistic theories of extra-sensory perception might be developed as science proceeds.

Mr Moncrieff, however, takes the other possible view, namely that the familiar forms of sensory perception might be treated as specialized aspects of extra-sensory perception, which is general and fundamental. This narrowing would have bio-psychological functions and be a product of organic evolution, which would account for the decrease in extra-sensory perception which is associated with the increase in efficiency of the sensory-perceptual functions in the higher organisms. He treats the whole matter in such a way as to support a philosophical theory of Direct Realism, and he shrewdly points out what is obvious enough to many people, that if Direct Realism is to be called 'Naïve', then nothing could be more naïve than some aspects of conventional scientific theories. At the same time it is apparent that nothing could be more naïve in some respects than the Direct Perception Theory itself. However, this way of thinking about extra-sensory perception is stimulating and might lead to some interesting changes in scientific as well as in philosophical thought, and it certainly deserves to be expounded and criticized.

Mr Moncrieff is a writer of engaging clarity who can make the most subtle matters easy to understand. The most difficult problems in his book, about 'Four-dimensionality' and its implications—which are far from being clearly handled—however, he proposes to discuss fully in a second and forthcoming work. It is only just to say that the text of the present book is marred by obscure abbreviations, such as 'M.O.', 'O.C.Q.', 'V.M.I.', 'P.S.O.', 'S.O.P.' and 'S.A.C.'. These force the reader unwillingly to refer back so often to p. 15, where they are explained, that it is obvious that they should have been printed always in full. Some of them then appear to be so ordinary that they did not deserve the distracting emphasis of special capitals. 'M.O.', for example, becomes 'mirror object'. There is also the difficulty in this book, as in so many works on the philosophy of perception, that it identifies perception far too much with vision. It would be interesting to think philosophically from the point of view of the other senses more fully, and to see if this led to any important changes in the theory.

R. W. PICKFORD

Skill and Age. An Experimental Approach. BY A. T. WELFORD. With a Foreword by SIR FREDERIC BARTLETT. London: Geoffrey Cumberlege, Oxford University Press, 1951. Pp. ix+161. 8s. 6d. net.

'This book is a report of the work done from 1946 to 1948 on Problems of Ageing by members of the Nuffield Research Unit attached to the Cambridge Psychological Laboratory. The names of the eight members of the Unit mainly responsible for the work are given on the cover, and a more detailed statement appears in the Preface.

The greater part of the book consists of a review of laboratory experiments carried out on groups of different age ranges. This is prefaced by a discussion of the possible effects of ageing on performance, and by a very short discussion of the nature of skilled activity, and is followed by an account of some investigations in various industries, intended to act as the beginnings of a field check on the laboratory findings.

The general attack on the problem of ageing is interesting. The author hopes to throw light both on the nature of skilled activity and on the problem of ageing, by investigating similarities and differences in the acquisition of skills in people of different age groups. The results, regarded as a basis for future work, are encouraging. The methods used do appear to show up consistent differences between age groups, and, though the differences found are not particularly new, it appears that they may be amenable to more concise definition by such methods. The book contains many extremely interesting comments on the nature of skills. Many of these, and especially those emphasizing receptor organization as a basic aspect, seem worthy of a more detailed discussion than the author has found space to give. The chief value of the book perhaps lies, as Sir Frederic Bartlett suggests in his Foreword, in its contributions to the understanding of the nature of skilled action, and in its methodological approach to ageing.

The industrial follow-up in the latter part of the book is less satisfactory. Although the interpretations made of the data collected broadly correspond with the laboratory findings, it would seem desirable to use some more convincing criterion than the mere numbers of different age groups surviving in particular industrial situations before placing much weight on this correspondence.

An Outline of Psycho-Analysis. BY SIGMUND FREUD. (New translation by James Strachey.) London: The Hogarth Press. 1949. Pp. ix+84. 8s. 6d. net.

This work represents the last of a series of attempts by Freud to give an overall account of psycho-analysis. It was begun in 1938 and never completed. Although it is incomplete it is, nevertheless, more compact and definitive than the nine or ten earlier works of Freud which had the same aim. For, coming as it did at the end of his life, the terms he uses are no longer maturing and developing in their meaning for him. This has the effect of making the book a source of definitions in Freud's own language. In fact the index in conjunction with the text could almost be used as a glossary.

Freud defined his aim in writing the book as follows:

'The aim of this brief work is to bring together the doctrines of psycho-analysis and to state them, as it were, dogmatically—in the most concise form and in the most positive terms. Its intention is naturally not to compel belief or to establish conviction....'

However, it must not be supposed that this is condensed or 'potted' Freud. As in all his works the restatements add something to the earlier statements. The text, too, is continuous, and shows perhaps more completely than any other of his writings, the inter-relatedness of the separate parts of his theory and summarizes the whole range of his views on particular subjects. Taking as he does a retrospective view, Freud has been able to put together in a page or two, for example, his views about the function of dreams or the nature of transference or factors in the Oedipus complex, such as its phylogenetic and its current significance, its importance for super-ego formation, the difference in its influence upon boys and upon girls, and so on. As with all Freud's writings, the more one brings to them the wiser and more far-reaching his implications seem to be. However, it would seem likely that *The Outline of Psycho-Analysis* would still be valuable where the earlier writings were not already familiar to the reader.

Mr Strachey's translation, like the others of his translations of Freud which have appeared since the end of the war, is a model of uncomplicated English. The uniform treatment of concepts and words made possible by the fact that the different volumes have a single translator, is in itself a tremendous gain, but the pointing of meaning which Mr Strachey achieves, sometimes with the help of a footnote, adds greatly to one's pleasure and ease in reading the book.

The Meaning and Practice of Psychotherapy. BY V. E. FISHER. London: Macmillan and Co. Ltd. (New York: D. van Nostrand Co. Inc.). 1950. Pp. xvi+411. 37s. and \$5.00.

Here is a delightful little book which bears the stamp of an experienced writer and teacher of psychotherapy. From the first page to the last there is not a dull paragraph, and it holds the reader's attention like a detective story.

The author is an eclectic with a Freudian bias. His interpretations are almost wholly Freudian, and yet, so well does he present his therapeutic principles, they will readily be understood, and very likely be accepted in their entirety, even by the student to whom this is his first introduction to psychotherapy. He is quite an exceptional author amongst present-day psychotherapists, for he scorns to use any but the most common and everyday psycho-analytic terms, and it is indeed a pleasant experience to read this clear exposition of psycho-analytic principle in simple, downright, unaffected and concise English.

The book is devoid of theory or rumination. Every principle is introduced through its practical application to a therapeutic situation. The style is tutorial but at no time does the author appear to lecture, rather does he present one with a verbatim record of clinical demonstrations. Another unusual feature is that the psychotherapy of every kind of psychotic patient is presented just as fully, and with just the same ease, as it is with psychoneurotic patients.

The author is obviously no respecter of orthodoxy. He even discusses when and when not to use a couch. He draws all kinds of therapeutic weapons out of the bag, so to speak, psychometric testing, vocational guidance, environmental re-adjustment and even medico-legal counselling, and links them up with a basic technique of short intensive psycho-analysis.

There is a very complete glossary as well as an index.

One could criticize the author's diagnostic groupings and many of his interpretations, but any such criticism would appear to be of a carping nature when ranged against the general merits of the work. This book should find a ready market amongst doctors, and also it is to be recommended to any student of psychotherapy whether he aspires to become a psychologist, psychiatric social worker, occupational therapist or mental nurse.

Textbook of Abnormal Psychology. BY C. LANDIS and M. M. BOLLES. Second edition. New York: The Macmillan Co. 1950. Pp. xii + 634. 37s. 6d. net.

The authors have brought their vast array of facts and theories up to date without losing either their simplicity of presentation or their freedom from bias. Relevant evidence is drawn from the biological and social sciences with a rare impartiality. The additional attention given to psychosomatic medicine and the physically handicapped remind us that this is the first post-war edition of this book. It is to be hoped that, as the need arises, there will be more of equal excellence. G.A.F.

Dementia Praecox or The Group of Schizophrenias. BY EUGEN BLEULER. New York: International Universities Press (London: George Allen and Unwin). 1951. Pp. xii + 548. 63s. net.

The publication of the first English translation of Bleuler's classical monograph *Dementia Praecox or the Group of Schizophrenias* is more than a most welcome psychiatric event. The thanks of all those English-speaking none-too-fluent readers of German will be offered to Dr Zinkin for his labours, for he has put at their disposal a book which must quickly reach the distinction of being one of the most frequently read text-books on the clinical aspects of schizophrenia.

These sections of the book are as fresh and clear as if they had been written to-day; indeed little, if anything, of major importance has been added since these descriptions of primary and secondary symptoms were given. The clarity of the subgroupings, according to the general lines of the Kraepelinian classification, serve as a timely reminder that, with the current preoccupation with forms of physical treatment, unknown to Bleuler, clinical diagnosis has tended at times to become perfunctory as far as the subgrouping is concerned, partly, perhaps, as a misinterpretation of the altered perspective of the relations between the subgroups indicated by Henderson and Gillespie's most valuable concept of the schizophrenia-paranoia series. Had Bleuler known and accepted the Kretschmerian personality typology, he would have found it helpful in clarifying the discussion of 'Schizophrenia and other Psychoses' (particularly 'the alcoholic state'). The section on treatment lacking, as it must, the modern physical developments, describes, in other respects, the goal still to be aimed at.

The format is pleasing and the index painstakingly done. This book should readily provide the translator and publishers with an occasion for reprinting, and this opportunity could perhaps be taken to correct the far from infrequent minor errors in grammar and spelling and other blemishes in the clarity of the translation. It is with much regret that these defects are mentioned but they do not obscure the excellent gift that Dr Zinkin has made. J.G.

Religion and the Cure of Souls in Jung's Psychology. BY HANS SCHAER. Translated by R. F. C. HULL. London: Routledge and Kegan Paul. 1951. Pp. vi + 226. 16s. net.

The Individual and his Religion. BY GORDON W. ALLPORT. London: Constable. 1951. Pp. xiv + 163. 12s. 6d. net.

It will be the best plan to allow the author of each of these books to be his own advocate. On pp. 213-14 Dr Schaer asserts, in his admirable exposition of Jung's ideas, that Freudian psycho-analysis wants to lead man to a sense of reality—as it understands reality—and to cure him of religion altogether, which it regards as superstition at bottom. Jung adopts a totally different view. He approves of religion as an essential manifestation of psychic life. There is no question of his regarding it as a colossal aberration or delusion, to be rejected on that account. At the same time he does not overlook the fact that all sorts of pathological phenomena can be found in religion, and that a pathology of religion would be easy to compile. But he refuses to accept the idea that religion is in itself a symptom of psychic morbidity. On the contrary, he sees in the religious function one of the most important elements of the human psyche. That is why he tries, as a psychologist, to help his patient along the road to the kind of religious experience he needs. If Freud wants to cure people of religion, Jung wants to cure them through religion, and to give a personal religion to each of them. The religious function is not the same for everybody. Religion may mean very different things to different people. To quite an extraordinary degree Jung is aware of the diversity in religious life, and tries to be as just as he can to every form it takes. Those who read his writings attentively, however, will notice after a time that he is inclined to be in favour of a specific form

of religion; this consists of living activities which are, on the one hand, essential components of human experience, and, on the other, happenings within some reality that transcends the individual—we could even say, happenings in God. Not that Jung ever goes so far as to say this. He would regard such an utterance as metaphysical, and metaphysics, in his view, is a pursuit which does not fit him as a psychologist.

Allport, on pp. 158–9 of his book, summarizes his ideas by stressing the varieties of religion, but he is more emphatically individual than Jung. His theme is the diversity of forms that subjective religion may assume. Many different desires may initiate the religious quest, desires as contrasting as fear and curiosity, gratitude and conformity. Men show a varying capacity to outgrow the religion of their childhood, and to evolve a well-differentiated mature religious sentiment. There are many degrees in the comprehensiveness of this sentiment and in its power to integrate life. There are different styles of doubting, different apperceptions of symbols, contrasting types of content that vary both with the culture and with the temperament and capacity of the believer. There are innumerable types of specific religious intention. How the individual justifies his faith is a variable matter, and the certitude he achieves is his alone. From its early beginnings to the end of the road the religious quest of the individual is solitary. Though he is socially interdependent with others in a thousand ways, yet no one else is able to provide him with the faith he evolves, or to prescribe for him his particular pact with the cosmos. A man's religion is the audacious bid he makes to bind himself to creation and to the Creator. It is his ultimate attempt to enlarge and to complete his own personality by finding the supreme context in which he rightly belongs.

It is striking that two authors so different in outlook as Jung and Gordon Allport should agree to so great an extent as would appear from these books. There are also striking differences. The reader who is interested in the psychology of religion will find much to think about in the two books, and Mr Hull is to be congratulated on his translation of the book about Jung.

R. W. PICKFORD

Patterns of Sexual Behaviour. BY CLELLAN S. FORD and FRANK A. BEACH. New York: Harper. 1951. Pp. x + 307. \$4.50.

Despite popular belief, the subject of sexual behaviour has been rather neglected by scientific psychology; many problems are unanswered and many more, presumably, are as yet unrecognized. This book is a pioneer attempt to bring together systematically the factual knowledge now scattered in the technical literature of three fields of work, physiology, animal psychology and cultural anthropology. The facts about primitive peoples are drawn from reports on 190 societies (well scattered over the major regions of the earth), and were located through the Human Relations Area Files; facts about complex modern society are confined to those reported for the U.S.A. by Kinsey and his few predecessors in the scientific study of sexual behaviour in America. In animal psychology the main emphasis is, of course, on the primates, though observations of other mammalian species are included.

The data from all three fields of study are assembled with a view to gaining, eventually, a better understanding of human sexual behaviour, but the authors carefully avoid facile explanatory transpositions from animal to human, primitive to advanced, or physiological to psychological. (One feels, however, the lack of a secure logic in the comparative use of animal psychology.) In the main they summarize and order the established facts and leave it at that; when, as occasionally happens, what they offer is speculation or debatable interpretation they make that clear. The book would be a valuable achievement and indispensable for future study of the subject if this were all.

In fact, however, a number of conclusions and general principles emerge from the survey. Several of them have a bearing on the subject of female sexuality. The authors show, for instance, that female sexual behaviour becomes progressively freed from hormonal control as one passes up the evolutionary scale, until in women no clear relation can be established between ovulation and sexual desire. This survey also shows a marked difference between male and female primates in the climax of coitus and points to the probable absence of orgasm in the females of sub-human species. A further difference between the sexes appears in the physiological evidence that the cerebral cortex plays a much greater part in the sexual responsiveness of males than of females in several sub-human species. A view of this biological perspective is likely to hasten the retreat from the phallogentric tendencies of early psychotherapy and of the generalizations based upon it. Other data confirm the familiar supposition of the importance of cultural influences in shaping the norms of sexual behaviour. Evidence from animal psychology and anthropology indicates that the so-called 'perversions' (a term well avoided by the authors in their effort after objectivity) are widely spread among animal species and human societies. There is also evidence that in the absence of adult discouragement children engage in a wide range of sexual activities from an early age and continue in them throughout childhood.

Personality and Psychosis. BY OTHO W. S. FITZGERALD. London: Baillière, Tindall and Cox. 1951. Pp. viii + 134. 12s. 6d. net.

Intended mainly for psychiatrists, this brief and stimulating book is also of interest to psychologists on account of its classification of pre-psychotic personality types. Psychiatrically it departs from orthodoxy by regarding manic and depressive psychoses as distinct clinical syndromes; psychologically it parallels this distinction by recognizing the 'impetuous extravert' and the 'deliberate extravert' as distinguishable personality types in addition to the introvert. It would appear that in Dr Fitzgerald's view all three types are the outcome of mutually exclusive inborn traits. Superimposed on them may be found any or all of three acquired personality traits, the hysterical, the obsessional and the paranoid. Dr Fitzgerald is careful to limit himself to 'the typing of the unstable pre-psychotic personality', but if his classification is valid for that purpose it is likely to have at least some relevance for the description of 'normal' and neurotic personality traits. The illustrative material offered by Dr Fitzgerald is interesting and persuasive though naturally not conclusive; whether or not his classification proves to be a tool of lasting value it undoubtedly deserves the attention of those who attempt any typing of personality. The author gives no list of references because he has wisely chosen to present his views briefly as the outcome of personal observations and without relating them to all the other work in this vast field; nevertheless, the book is modest in tone and offers its original viewpoint without a blare of trumpets. The index is usable once the secret of subtracting 4 from each page reference has been mastered, and this will not be beyond readers of the calibre for whom the book is intended.

Technique of Psycho-analytic Therapy. BY SANDOR LORAND. London: George Allen and Unwin. 1951. Pp. viii + 251. 12s. 6d. net.

Clinical Studies in Psycho-analysis. BY SANDOR LORAND. New York: International Universities Press. 1950. Pp. 272. \$4.00 net.

Technique of Psycho-analytic Therapy is a practical book on the technique of analysis, intended for analysts in training, and richly illustrated with case material. *Clinical Studies in Psycho-analysis* is more miscellaneous, consisting partly of case histories and essays on particular analytic problems, and partly of more general essays, such as that on the Correlation of Theory and Technique. There is a certain amount of overlapping between the two books.

Lorand is at his best when dealing with technique. Compared with some of the more rigid and old-fashioned Freudian analysts, he is elastic about details, and he follows his master Ferenczi in taking an active attitude to his patients at certain stages of analysis, and giving them advice and suggestions. He points out that complete passivity on the part of the analyst is impossible anyhow, since the patient will conceive of his passivity as an active measure and react to it as such. He has worked out in considerable detail the variations in technique that suit different kinds of patient. Anxious patients must be encouraged to face the situations of which they are afraid. With compulsion neurotics one must work slowly and avoid giving suggestions. Depressed patients in the earlier stages of treatment need a great deal of advice, reassurance and general kindness. And so on.

Lorand understands sexual problems well and expresses himself fluently in the language of physical sexuality. His contribution to the problem of vaginal orgasm is noteworthy. He is less good at conveying qualities of feeling; for example, he never quite manages to express just how a patient benefits from getting angry with his analyst, although an important part of his method is to make his patients angry by forbidding their more childish pleasures.

He has an open mind as to the stage at which the super-ego comes into being, and thinks it is not very important to determine the exact course of the development of object relationships in the infant. He gives an impression, however, of being too little aware of the extent to which bad behaviour is due to a revolt against a severe conscience. Children and criminals are not 'freely impulsive'.

Lorand is at his weakest in dream interpretation. He does not seem fully to understand the purposive nature of the dream, nor to pay sufficient attention to detail. For example, in a certain pair of dreams he is content to take the bath-robe, the turkey and the fur coats as penis symbols without seeking to understand why the patient's unconscious should have chosen just those symbols at that time. One cannot be certain without knowing the patient, but it might well be that the bath-robe was her own excessive masculinity which drove her to seek an unsatisfactory lover, the bath a washing away of this, while the personality of the woman friend would probably be a guide to the changes she needed to make in herself. At any rate every detail in a dream has meaning, and the more the analyst can explain the details the finer his sensitivity to his patients' emotional experience will become.

JANE DARROCH

Neurosis and Human Growth. BY KAREN HORNEY. London: Routledge and Kegan Paul. 1951. Pp. 391. 21s. net.

Dr Horney's latest book is intended to formulate a more complete theory of neurosis than that previously advanced by *Our Inner Conflicts* and her other publications. The author views the onset of neurosis as a reaction to an unfavourable environment in which the individual develops a need to remedy certain apparent deficiencies in himself, and becomes impelled to lift himself above others to satisfy this need. The neurotic therefore embarks upon a *search for glory*, and creates in his mind an *idealized self*, which thereafter drives him to try to achieve impossible perfectionist standards of behaviour. In this way the real self is stifled, the individual's inner feelings recede in importance, and a progressive *alienation from the self* ensues with the neurotic becoming more and more a spectator of his ordinary self, and measuring its performance against the perfectionist standards demanded by the idealized self.

The real merit of this thesis emerges in providing a first-rate explanation of phenomena generally labelled 'masochistic', while simultaneously accounting for the confusing ambivalence of neurotic intrapsychic processes. The author writes with great clarity and employs self-explanatory terminology throughout—a welcome feature found in few psycho-analytic publications. Many readers, however, may question whether there are so many real differences between *self* and *idealized self* and the Freudian concepts of *ego* and *superego*, and may consider the latter concepts capable of modification to cope with advances made by Dr Horney, especially with regard to the structure and function of the superego. Also open to doubt is the author's criterion of morality, as she appears to identify moral goodness with healthy social adjustment. Many examples of neurotic behaviour are drawn from a more intellectual group of neurotics of higher social class than that found in the run of ordinary clinical practice, and this may partly account for the interesting classification of neurotic types appearing somewhat incomplete. Upon the whole, the book forms a sound treatise upon the intrapsychic processes of neurotics, and represents a further step towards more objective criticism, evaluation, and re-integration of psycho-analytic hypotheses.

W. A. MCELROY

Thomas W. Salmon, Psychiatrist. BY EARL D. BOND. London: Chapman and Hall 1950. Pp. 237. 15s. net.

Dr Thomas W. Salmon became a psychiatrist by accident in 1903 when, as a member of the Public Health Service, he was set to plan the mental examination of would-be immigrants at Ellis Island, his humane outlook on the problems that offered themselves involving him at once in a fight against 'official attitudes', a fight that kept him busy for the rest of his life. In 1908 Clifford Beers's notable book *A Mind That Found Itself* led to the formation of a National Committee for Mental Hygiene of which Salmon was appointed secretary and, making public the lamentable lack of care and provision for the mentally afflicted, he enlisted powerful support to remedy that lack. The English reader may find special interest in Salmon's work as Director of Psychiatry with the American Expeditionary Force in 1917, for his problems were the same as ours, and our army psychiatrists in the latest War were familiar with the methods he devised and described for handling them. He was able to claim that benefit to the army resulted from the early rejection of men on psychiatric grounds—a procedure that was strongly resisted when our English psychiatrists put it into force years later.

Salmon became spokesman for the mentally disabled veteran and an advocate of the psychiatric approach to delinquents, to difficult children, and to their parents. The American Psychiatric Association condoned his unorthodox entry into their circle by making him president for 1923-4, and the leading psychiatrists of Great Britain and America have shared in the T. W. Salmon Memorial Lectures, given annually in the Academy of Medicine in New York City. Dr Bond has given us an intimate picture of a gallant fighter, with his strength and weaknesses and, what is historically important, the story of the part his activities played at a crucial time in the development of a public sense of responsibility for the welfare of the mentally afflicted.

M.C.

Principles of General Psychopathology. By SIEGFRIED FISCHER, M.D. New York Philosophical Library. 1950. Pp. xxiv+327. \$4.75.

The title of this book modestly declares it to be a work on psycho-pathology, but it contains a comprehensive survey of all those features of normal psychology which are pertinent to psycho-pathology, and it devotes a deal of space to the psychiatric application of psycho-pathological concepts.

The Author presents his work as a stimulus to the student to appreciate the problems of psychopathology, rather than a treatise on facts and solutions. The thesis, therefore, especially in the earlier chapters, is philosophically orientated to a degree which appeals only to the advanced student. The style suggests that the author is as erudite in his subject as he is lacking in confidence in the English language. Many an accepted term is repeatedly followed by the German equivalent in brackets, thus, Reference (*Zuordnung*), Incoherence (*Zerfahrenheit*), Prolivity (*Weitschweifigkeit*), Awareness (*Gegenstandsbewusstsein*) and Impulse (*Antrieb*). This uncertainty about terminology has the unfortunate effect of causing the reader frequently to doubt whether he has grasped the author's concepts. Although the definitions used are, in general, concise and clear, they are not so in all instances, and that deficiency is a surprising one since the preface contains an admonition in support of the exact use of expressions.

An original viewpoint is claimed in respect of certain subjects such as a distinction between consciousness and awareness, some interpretations of the pathology of thought, and the definition of intelligence.

In regard to consciousness the definition of E. Husserl (1913) is accepted, namely, that it is 'the total real phenomenological condition of the empirical ego, or as the interweaving of the mental experience into the entity of the stream of psychological experiences'. At first sight this rather ponderous definition seems to mean no more than that consciousness comprises all that one is aware of at any moment. As one reads on, however, it becomes apparent that awareness is differentiated from consciousness. Whereas consciousness incorporates both objective and subjective experience, awareness is concerned only with the objective elements of total experience. Awareness, in other words, is concerned with objects, real or unreal (i.e. percepts or images) which are the focal point of immediate interest or attention. This distinction between consciousness and awareness serves to emphasize the author's rejection of the Freudian concept of a pre-conscious. The several illustrative diagrams, however, do not convey any convincing arguments in favour of recognizing two levels of consciousness and one of unconsciousness, as against the Freudian concept of a conscious, pre-conscious and unconscious mind.

In regard to pathological thought a concept called *Sachverhalte* is introduced. The immediate definition of *Sachverhalte* does not quite tally with the subsequent use of the term, so one has to read the whole book to grasp its meaning. It concerns the recognition of relationships between objects. Admittedly there is no English equivalent of *Sachverhalte*, but the more familiar German term, *Gestaltqualität*, would seem to serve the purpose equally well.

Disorders of thought, on the whole, are classified fully in the orthodox manner, but a distinction is drawn between prolixity, circumstantial thought and flight of ideas. The definition of intelligence is bravely attempted. 'Intelligence is the ability to abstract and to apprehend *Sachverhalte* (to construct relations) purposively and correctly as well as the ability to pose problems.' This definition is not nearly so impressive as the author's criticism of his own definition, and the preamble in which he sets out all the many difficulties in the way of defining intelligence. Volitional action is distinguished from 'act of will'. The distinction is not mere academic hair-splitting because it is put to practical use in the description of catatonic behaviour. Instinct and libido are not mentioned in the usual sequence, but the term 'drives' is used synonymously with instinct. In contrast to the general attention to detail, 'drives' are given little more than a passing reference.

On the whole, the author declares himself to be in agreement with psycho-analytic principles, with only a few but notable divergences. On the subject of repression, for example, he states that the psychoanalysts are wrong in maintaining that unconscious tendencies dominate our behaviour. In repression of desire, he states, the intention towards the object of desire ceases so that the individual ceases to strive, but, in contrast to that, where the object (as distinct from desire) of a drive is repressed, the desire continues as a blind striving towards the repressed and therefore unknown object. This is a difficult argument to follow because the term repression seems to be applied indiscriminately to both suppression and repression. It is improbable that the average psychoanalyst would be convinced by the argument that unawareness of striving is a valid criterion on which to suppose the extinction of desire.

The final chapters, dealing mainly with psychiatric and neurological applications, provide the student with much easier reading material than the opening chapters on psychology. The author's original viewpoints are somewhat clouded by a laborious style of expression, but such disadvantages are in large measure offset by his concise text-book tabulations, and his unflinching plan of linking every theoretical point to a clinical observation.

Sex Variants. A Study of Homosexual Patterns. By GEORGE W. HENRY. London: Cassell. 1951. Pp. xiv + 1130. 42s. net.

This book is sponsored by the Committee for the Study of Sex Variants, set up in New York in 1935 to correlate work on the subject, to stimulate research, and to disseminate information. It approaches the subject from the physical, endocrine, and Roentgenological aspects, as well as the sociological and the psychological.

The introduction deals with the attitude of the community towards the sexually abnormal. It reveals that in the United States the legal outlook is often harsh, even to the extent of depriving the offender of civil rights such as the vote, and ex-service men of all pension benefits.

In the body of the book there are detailed case studies of eighty cases of abnormal men and women, each class being divided into Bisexual, Homosexual, and Narcissistic cases. These studies are very complete, describing the individual from many aspects—physical, mental, social, and so on. The Appendix gives general impressions, results of Masculinity-Femininity Tests (on the Terman and Miles plan), physical characteristics of the two sexes, anthropological data, pelvic measurements, and the gynaecology of homosexuality. There are several pages of drawings of the sexual organs of the subjects examined.

This book represents a great deal of labour. The results will be of interest to specialists in the field of sexology, but its appeal will be limited to them, and perhaps anthropologists. A note on the fly-leaf states that the book has been prepared for the use of the medical and allied professions only.

The Nature-Nurture Controversy. By NICHOLAS PASTORE. King's Crown Press (London: Geoffrey Cumberlege). 1949. Pp. xvi + 213. 18s. net.

The object of this book is to investigate the relationship between the outlook of scientists on the nature-nurture controversy and their attitudes towards social, political and economic problems. If a close relationship should be found, then the question of causal relationships will be considered. It has too often been assumed that a causal relationship does exist.

The nature-nurture controversy became a prominent feature of scientific investigation after 1900, and this period was marked by wide social and political changes, illustrated by the rise of democracy and the growth of socialism. The philosophy underlying social reform is environmentalism; the opposite point of view asserts that social evils could only be eliminated by a change in the innate characteristics of man.

The controversy is an old one, dating for Western thought from Plato and Aristotle, and continued through the writings of Helvetius, Diderot, Rousseau and Condorcet. From the middle of the nineteenth century the problem ceased to belong to science and spread to the application of science to social philosophy, education, philanthropy and both national and international politics.

The controversy is in a different category from many scientific problems in that the relevant data do not afford a decisive formulation in favour either of heredity or environment, although attempts have been made to interpret the evidence so as to emphasize the all-importance of one factor or the other. It may, however, be that the solution to the controversy lies in the manner in which the scientific judgments are influenced by factors extraneous to the scientific situation, i.e. the controversy is essentially 'sociological'.

The writer has made a study of twenty-four American and English scientists prominent in this controversy during the period 1900–40. Individuals for study were selected because of their significant relationship to the nature-nurture controversy, either by way of initiating different points of view, or by furthering relevant research. Only those who expressed themselves on both nature-nurture issues as well as controversial social and political questions were considered for selection. Care was taken to see that the selections from each author's writings were really typical of his attitude to this controversy.

The following six English writers were selected for study: Francis Galton, Karl Pearson, William Bateson, William McDougall, Lancelot Hogben, J. B. S. Haldane, and in addition eighteen American writers.

It is recognized that a possible source of error, in addition to errors of sampling, is that written productions only could be studied. Part II consists of a study of the writings of the twenty-four scientists with quotations from their published works and explanatory notes linking the quotations. Part III gives a summary and conclusions.

The writer is of the opinion that the sociopolitical allegiances of the scientists were a significant determinant of their position, influencing the formulation of their hypotheses, the method of verification, the

conclusions drawn from the investigation and the statement of the implications of these conclusions for society. He adds that in most cases the scientists were not aware of these influences.

The method adopted is interesting and the book should suggest a similar application to other problems of an analogous nature. M.S.

Measurements of Mind and Matter. By G. W. SCOTT BLAIR. London: Dobson. 1950. Pp. 116. 9s. 6d. net.

Dr Scott Blair's aim in this book, which he dedicates to 'all those who are not afraid to cross frontiers', is to formulate a unified Theory of Measurement which could apply equally to the phenomena studied by psychologists and by physical scientists. To the possibility of such a *rapprochement* the author attaches considerable importance, for the general improvement of scientific, and perhaps, by implication, even of international relations. The book is not written from a psychological standpoint, and the psychological reader may be a little surprised to find such diverse matters as the Weber-Fechner law, the principles of Factorial Analysis and Rorschach all subsumed under a single chapter heading. Other sections of the argument, which is largely concerned with the comparison of values of incommensurables, exhibit a comparable (and legitimate) degree of compression, and this may perhaps render the book more difficult than the author had intended. Nevertheless, it is a vigorous and provocative essay, and well calculated to add the reader to the number of those qualified for inclusion in the dedication. B.S.

Annual Review of Psychology. Edited by CALVIN P. STONE and DONALD W. TAYLOR. Vol. II. Stanford, California: Annual Reviews Inc. (London. H. K. Lewis and Co. Ltd.). 1951. Pp. ix+389. \$6.00 net.

The decision to include Psychology in the 'major fields of science' in which annual reviews of current literature would be published, is one for which psychologists will be grateful. As the volume of current literature grows larger, there is a growing need for critical and informed reviews to enable one to keep some grasp on work in other than one's specialized fields of interest, and this would appear to be the main value of this series.

The reviews, of which there are eighteen in this volume, make no claim to be comprehensive, but are 'interpretative and evaluative'. On the whole, the various authors succeed in following this instruction from the editors. It is a weakness of some articles, however, that only American literature has been reviewed. This has, in certain cases, reduced the breadth of problems considered important. In some cases, too, the authors have employed such highly technical language that specialized knowledge is required to understand it. As reviews of current literature can contribute little to the man already expert in the particular field, the value of such articles would be greatly increased with more attention to the manner of presentation.

This is, nevertheless, a valuable book which should prove most useful both to those who have to teach psychology and to senior students.

Scientific Thought in the Twentieth Century. Edited by A. E. HEATH. London: Watts. 1951. Pp. xvi+387. 42s. net.

This is, on the whole, a disappointing book. Its defects do not arise from the contributions themselves, but are inevitable in any book which attempts to cover so much ground in so short a space. There are fourteen papers ranging from the Philosophy of Science through Physics and Chemistry to Social Anthropology and Sociology with General Medicine and Social Medicine looking rather odd in such a context.

The papers themselves, so far as the reviewer can assess them, are sound and factual. On the whole their tendency is to give a brief outline of recent history in their subject, but occasionally something more systematic is attempted. In particular, A. J. Ayer's paper in Philosophy of Science is a nice condensed piece of logical writing. The presence of some distinguished elder statesmen such as R. A. Fisher in Statistics, Sir Cyril Burt in Psychology, while it has given the book weight and authority has also given it a slightly retrospective slant, and the question 'Where do we go from here?' is not always easy to answer from a study of its pages. One is left, too, with the feeling that only a few people could read and understand all the papers or even most of them, and the collection seems to have been made without any real

public in mind. 'This book has been designed' says the blurb 'to provide the general reader with an authoritative account of development in scientific thought during the first fifty years of this century'. The account is certainly authoritative, but the general reader is going to find that he is expected to bring to his reading a background of knowledge that is both very wide and rather technical. J.D.

Proceedings of the Second International Congress on Orthopedagogics. Edited by I. C. VAN HOUTE and BERTHOLD STOKVIS. Amsterdam: Systemen Keesing. 1950. Pp. xii + 534.

The proceedings recorded in this report show a growing need for remedial education among children with mental and physical handicaps. Though many of the papers relate clearly and briefly the extent to which the need has, and has not, been met in the past, few refer to the results of specific experiments carried out during the ten years which have elapsed since 1939. A remark made by the chairman in his opening speech, '...Sad to say, our work cannot be said to yield any better results now-a-days than it did ten years ago', is reflected in many of the papers and discussions, which tend to be records of pious hopes rather than actual accomplishments.

Despite such pessimistic comment, however, there are indications that a change in orientation has taken place among those responsible for the guiding principles of remedial education. The trend throughout, and to a greater extent than formerly, is to emphasize the sociological aspects of special education. There is an increased recognition that such education must be planned with the knowledge that, while many defects in themselves cannot be made good, a fuller recognition of potential assets which remain will often suggest more fruitful lines of work.

Suggestions are made, both implicitly and explicitly, for future research in connexion with problems of partial defects.

In particular two issues are raised, viz.:

- (a) To what extent are defects primary and to what extent only particular expressions of underlying, but more general, abnormal conditions?

and

- (b) To what extent do primary defects react on other functions with which, in a normal subject, the disturbed or undeveloped activity forms a functional unity?

M.I.D.

Human Engineering. LAWRENCE E. ABT. Volume 51, Art. 7 of the *Annals of the New York Academy of Sciences*, 1951. Pp. 155. \$2.75 net.

According to Leonard C. Mead, who contributes the opening article in this volume, 'human engineering is that endeavour which seeks to match human beings with modern machines so that their combined output will be comfortable, safe, and more efficient'. There are thirteen articles, on such diverse themes as aeromedical research, lighting, prosthetic devices, and dial-reading; indeed, less than half the book has any direct connexion with psychology. Perhaps it is to redress the balance that John L. Kennedy begins his article on the *Handbooks of Human Engineering Data* being prepared at Tufts College by remarking on the astronomical number of publications on experimental psychology now in existence; adding, by way of a non-sequitur, that 'it is evident that a large body of factual information has been obtained by psychologists'. Ross McFarland surveys superficially but pleasantly some aspects of transport aircraft design in relation to 'pilot error' and other weaknesses of the flesh and the spirit. Henry Hausner's paper on the effects of lighting on efficiency is enjoyable, as also is that of Clifford Seitz on cockpit lighting. Alphonse Chapanis discusses the analysis of errors by means of the variance addition theorem in what would form an excellent chapter in a beginners' textbook of statistics. The remaining psychological contribution is a report by William E. Kappauf and W. M. Smith on a dial-reading experiment.

W.E.H.

Infant Speech: A Study of the Beginnings of Language. By M. M. LEWIS. Second edition, with additional chapters and appendices. London: The International Library of Psychology, Philosophy and Scientific Methods, Routledge and Kegan Paul. 1951. Pp. xii+383. 21s. net.

This reprint of an important book includes two new chapters (with appendices giving the data on which they are based) which have already appeared as papers in the *British Journal of Educational Psychology* (1936 and 1937). They deal with the beginning of the infant's reference to past and future and with the early functions of his questions. The treatment of the latter topic is particularly interesting for the emphasis placed on the early use of the question as a means of securing social interplay, long before the child really wants information. Here no doubt the social psychologist concerned with adult conversation will find a rudimentary form of later functions. Dr Lewis's discussion of the fascinating 'Why?'-questions' and the implication of the infant's use of 'because' puts subtle points in brief compass and by implication suggests lines of further research.

Your Child Makes Sense. By EDITH BUXBAUM. With a Contribution by FLORENCE L. SWANSON, M.D., and Foreword by ANNA FREUD. London: George Allen and Unwin. 1951. Pp. xvi+204. 12s. 6d. net.

'A Guidebook for Parents' is the sub-title of this publication. The author shows how children's spontaneous activities at each developmental stage 'make sense' when seen as efforts to satisfy the instinctual needs which are uppermost at the time. She advises upon the ways in which gratification and educational control should be blended in dealing with these activities. The book is fully illustrated with examples, taken from Dr Buxbaum's observations as a child analyst, of mishandling and its consequences.

The book is in three parts. In Part I a chapter has been contributed by Dr Florence Swanson on 'Physical Development and Care of the Child', dealing with this aspect from conception onwards. This chapter should allay many of the anxieties which parents normally experience in pregnancy and after the child's birth. There follows a chapter on the early relation between mother and child, stressing its importance in character formation. Part II deals with the oral, anal and phallic phases of instinctual development, and Part III concerns social development in the family, school and cultural group.

It is a pity that the book is so heavily weighted on the negative side with warnings concerning mishandling and possible resultant problems. Anxious parents are likely to be unduly worried by the cautionary tales. A more positive approach would have been desirable in a 'guidebook'. Nevertheless, the content of the book is based on sound principles, and, to stable and educated parents, should prove instructive and interesting.

A Group of Juniors. By FRANCES TUSTIN. London: Heinemann. 1951. Pp. 90. 6s. net.

The author of this little book had unusual opportunities to observe the behaviour of a dozen young children between the ages of 8 and 10½ in a progressive co-educational boarding school. After a brief account of their background she presents 'The Diary'—a record of their play activities, conversations and drawings over a period of two months. This is not a description of their behaviour but a verbatim record of their conversation and jottings about what each child was doing during the periods of observation. Though this section is somewhat jerky to read straight through, a clear picture of the personalities of the members of the group emerges. The significance of their make-believe and group play is then discussed. The author regards their co-operation in imaginative play as an expression of group fantasy. Not all readers will agree with some of her interpretations, but the value of play in releasing aggression, in the working out of conflicts and expressing needs, and in developing social attitudes is clearly shown through concrete examples. Most of the children studied were of above average intelligence and their background atypical. This the author realizes and she stresses the need for further studies of this nature with children from different home and school environments.

A.H.D.T.

Psychologie du Premier Âge. By MARCEL BERGERON. Paris: Presses Universitaires de France. 1951. Pp. iv+141. 240 fr. net.

This is a study of childhood from birth to the third birthday. Whilst the child is viewed as a whole, a standpoint that is insisted on throughout the book, for convenience his development is separated into its physical, intellectual and social and emotional aspects, but the author has the same difficulty as writers in other countries in that the amount of research material available in these three spheres is widely different; there is still poverty in the emotional field and the study of the infant 'dans son milieu' has far to go. Dr Bergeron inevitably leans heavily on Gessell's work and transports the American milestones to his own country, which will, I suspect, cause some distress amongst French mothers when the question of the age of walking is in issue—in Europe we stubbornly seem to walk some few months later than in the States.

Very wisely there is no attempt to synthesize the work of Piaget and Freud, but their contributions receive considerable weight as does that of M. Wallon which deserves to be better known in this country. It is indeed under his influence that the whole book has been written and quite a sound one it is: 'l'objet de la psychologie est de considérer l'homme en contact avec les choses et en contact avec les autres hommes.'

Final chapters are concerned with practical applications in the medical and educational spheres and quite acceptable they are from all points of view—if somewhat dictatorial at times: 'jamais les parents ne devront abandonner leurs enfants aux domestiques'.

J. M. GIBBS

A Dictionary of Speech Pathology and Therapy. Compiled by SAMUEL D. ROBBINS. Cambridge, Mass.: Sci.-Art Publishers. 1951. Pp. 112. Paper-bound \$1.65; Library text \$2.70.

Nobody could be expected to read this book through, but a lot of people should find it an extremely useful book of reference. The verbalization of experts who write about speech defects and their treatment is often distractingly difficult and varied. Here there is first a very good classification of disorders of speech and language into twelve principal groups; then a concise key to the dictionary which gives a quite admirable indication of how it can best be used, and finally the dictionary itself, as to which it is claimed that 'every term in the literature of the subject is explained clearly and precisely and its pronunciation indicated'. Certainly, so far as the present reviewer goes, every technical term that he has thought of looking up he has found without difficulty, and there is an immense number that he did not know at all. Whether the group of subjects concerned really needs so elaborate and, in many instances, so odd a collection of technical terms may remain a matter for dispute. But anyway they have them, and that being the case, this book represents a much needed enterprise. It should be added that the volume is remarkably well printed and produced.

F.C.B.

OTHER PUBLICATIONS RECEIVED

- Bibliographies in the Social Sciences. A Selected Inventory of Periodical Publications.* Paris: 1951. U.N.E.S.C.O. Publication 798. Pp. 129.
- FAITHFULL, THEODORE. *A Handbook of Sex Education.* London and Birmingham: Rylee Limited. The Thesis Library. 1950. Pp. 115. 8s. net.
- LOWREY, LAWSON G. *Psychiatry for Social Workers.* Second edition. Columbia University Press (London: Geoffrey Cumberlege). 1950. Pp. xviii+385. 30s. net.
- MASSON-OURSSEL, PAUL. *La Pensée en Orient.* Paris: Armand Colin. 1949. Pp. 216. 180 fr. net.
- MEDUNA, L. J. *Carbon Dioxide Therapy. A Neurophysiological Treatment of Nervous Disorders.* Springfield, Illinois: Charles C. Thomas (Blackwell Scientific Publications). 1950. Pp. xvi+237. Oxford: 36s. net.
- Organizacija Rada* (Organization of Labour). A Monthly Review. Nos. 1-3, 1951. Belgrade.
- SERRATE TORRENTE, AUGUSTIN. *Contribucion al Estudio de la Prueba de Behn-Eschenburg (Behn-Rorschach) en Psiquiatria.* Zaragoza: Heraldo de Aragon. 1951. Pp. 149.

CORRESPONDENCE

THE EDITOR,

British Journal of Psychology (General Section)

Dear Sir,

I should like to raise two points of controversy with Dr H. J. Eysenck on his paper entitled 'Primary social attitudes and the "social insight" test', which appeared in your March and May number, 1951.

My first objection is to the use of the terms 'tender-mindedness' and 'tough-mindedness'. I submit that we regard as 'tender-minded' a person who is, in general, very distressed at the thought of the infliction of pain on another; yet, according to the published figures, the people whom Dr Eysenck calls 'tender-minded' believe that 'people suffering from incurable diseases should *not* have the choice of being put painlessly to death'. They are fairly indifferent to the 'dropping of the first atom bomb', 'blood sports', and 'the torturing of an enemy'; and although they are slightly against the flogging of violent criminals they are slightly in favour of beating children.

What are the distinguishing characteristics of Dr Eysenck's 'tender-minded'? They are most strongly in favour of two propositions:

- (7) 1. Only by going back to religion can civilization hope to survive; and
- (27) 2. It is right and proper that religious education in schools should be compulsory.

They strongly disagree with the following propositions:

- (23) 1. Divorce laws should be altered to make divorce easier.
- (16) 2. The laws against abortion should be abolished.
- (28) 3. Men and women have the right to find out whether they are sexually suited before marriage (e.g. by trial marriage).
- (3) 4. Persons with serious hereditary defects and diseases should be compulsorily sterilized.
- (40) 5. There is no survival of any kind after death.
- (9) 6. Sunday observance is old-fashioned and should cease to govern our behaviour.
- (21) 7. People suffering from incurable diseases should have the choice of being put painlessly to death.

(The numbers in brackets are those of the statements in the Inventory of Social Attitudes.)

Surely what is depicted here is the typical 'Christian', if not Roman Catholic, attitude? 'Religious' or 'mystic', perhaps, but certainly not 'tender-minded'. And its reverse could be termed 'agnostic' or 'rationalistic'; perhaps even 'scientific'; but not 'tough-minded'.

My second point concerns the use of the Word Connexion List as a measure of neuroticism. Here I am at a disadvantage, as I do not subscribe to *J. Personal.*, *J. Soc. Psychol.* or *Int. J. Opin. Attit. Res.*, so I am ignorant of the validity and reliability of this test. Nevertheless, I am of course prepared to accept Dr Eysenck's statement that they 'are high enough for group work, though not for individual diagnosis.' A correlation of 0.152, even if highly significant, implies that no more than 2 $\frac{1}{4}$ % of the variance of one of the variables is accounted for by the variance of the other; and if one of them has not a high validity with its criterion it is riskier still to assume the existence of a significant correlation between the *other* variable and that criterion.

I therefore beg leave to doubt the statement that 'neuroticism is correlated with tough-mindedness' (or, as I should prefer, 'with rationalistic-mindedness'). Is the

regression between these two variables linear? May not extremely religious people, equally with extremely rationalistic, be neurotic?

Yours faithfully,
LEONARD J. HOLMAN

9 Ryden's Way,
Old Woking, Surrey.
27 May 1951

THE EDITOR,
British Journal of Psychology (General Section)

Dear Sir,

Mr Holman's first point is essentially a semantic one. He may be right in his submission 'that we regard as "tender-minded" a person who is, in general, very distressed at the thought of the infliction of pain on another'; however, in the series of papers of which the first pages of the article in question are only a brief summary, I pointed out that the terms 'tough' and 'tender-minded' were borrowed from William James, who also used them in a sense somewhat removed from their popular meaning. I also pointed out that the terms used were probably not the best ones to be used, but that in the absence of anything better they would have to serve until the nature of the factor in question had been clarified a little further. Like other scientific concepts, a factor must be operationally defined; if that can be accomplished, the 'naming' process is of subsidiary importance. Mr Holman's suggestion that the factor should be re-named a 'religious' or 'Christian' factor overlooks the fact that the religious items characterize the *second* quadrant only, and are as highly correlated with the conservative as with the tender-minded factor. Nor do I see how the reverse of this factor could be called 'rationalistic' or 'scientific'; anti-semitism, ethnocentrism, and the belief that war is innate do not by and large appear characteristic beliefs of the rationalist or the scientist!

Mr Holman's second point would appear to involve a statistical fallacy. A low but significant correlation between tough-mindedness and neuroticism *gains*, rather than loses, in importance through lack of perfect reliability and validity in the two tests; perhaps I may refer Mr Holman to the usual 'correction for attenuation' formula for proof. If two tests measure variables *A* and *B* respectively with much less than perfect reliability and validity, then the correlation between the tests will *ceteris paribus* be considerably lower than the 'true' correlation between *A* and *B*. Corrections can be made, of course, to make the empirically observed correlation approach the 'true' correlation; I have not done so in order to avoid overinterpreting a relatively minor finding to which I only devoted four lines in my paper. Regarding Mr Holman's question at the end of his letter, it may be stated that the regression between the two variables did not depart significantly from linearity.

Yours sincerely,
H. J. EYSENCK

University of London,
Institute of Psychiatry.

